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LECTURES
ON
MEDICAL JURISPRUDENCE,
NOW IN COURSE OF DELIVERY
AT THE
UNIVERSITY OF LONDON.

BY PROFESSOR A. T. THOMSON.

LECTURE XV.

SIGNS OF PREGNANCY AFFORDED POST-MORTEM. —*Impossibility of proving Conception immediately after Intercourse.—State of the Ovary soon after Conception.—Question where the Ovum is Impregnated; Opinion in favour of Ovarian Conception.—Question of the Corpora Lutea respecting Virginity. Opinion that the real Corpus Luteum always evidences Conception. State of the Uterus in the earliest Stage of Pregnancy; how to search for a Fœtus. Appearances up to the end of the third Month; phenomenon of Quickening. Appearances during each subsequent Month.—Appearances indicating Abortion or Delivery.—Miss Burns' Case. —The least equivocal Post-Mortem Sign of Delivery.—Grounds for Conjecture in early Pregnancy.*

GENTLEMEN:—Having examined the proofs of the existence of pregnancy in the living subject, when it becomes an object of medico-legal inquiry, we have now to investigate the proofs which dissection affords when the female is dead; as, for instance, in cases of suicide, homicide, or sudden death from any cause. In pursuing this inquiry I shall divide the investigation into three heads:—

1. What are the appearances, in the dead subject, which indicate pregnancy in the first month of uterine gestation?

2. What in each succeeding month?

3. What are the appearances which indicate that delivery has recently happened at any period after quickening?

In pursuing our first inquiry, the question may be asked,—Suppose a female is murdered by her paramour immediately after coition, or suppose a married woman is caught in the act of adultery by her husband, and is killed, are there any physical evidences, in a post-mortem examination of the body, supposing that conception had taken place, to prove the act of sexual intercourse, or the fact of conception?

The celebrated RUYSCH has recorded two cases of this kind which fell under his observation. In the second case, which is in the sixth book of the *Thesaurus Anatomicus*, he says, "being called upon to open the body I found the uterus somewhat more tumid than natural, and, therefore, I suspected that the woman had conceived, and on opening it I found the substance to be thicker, and in the cavity of it, as well as in that of both the tubes, something was lodged very closely resembling the seminal fluid." Now, notwithstanding the high authority of RUYSCH, I believe that no inference can be drawn from these signs, unless, as in the cases described by that distinguished anatomist, the circumstances of the previous sexual intercourse be so well known, that no doubt can remain that some change must have taken place in the uterine organs, and then the symptoms described by RUYSCH may be justly regarded as corroborative evidence. Indeed, it is certain that no proof of the act of coition can be derived from the condition of the uterus, even if conception take place, when the female is murdered during the act, or immediately after it. No medical witness would be justified in offering an opinion on the subject, so far as the proofs are derived from the uterus, or the ovarium, as I shall presently explain to you. It is, therefore, in my opinion, impossible to determine the fact of conception immediately after that of coition. But let us suppose that ten days, or more, have elapsed, what then are we to look for? what are we to consider as the infallible sign of conception at the earliest period?

1. What are the appearances of those parts of the generative system which are directly the seat of conception, at the earliest period? To understand this question properly we have to look at the effects of conception upon the condition not only of the

uterus, but, more especially, of the ovarium. In examining the ovary of a woman who is supposed to have lately conceived, what do we expect to see? 1. An inequality, or small tumefaction, on its surface, with a distinct cicatrix. 2. On cutting open the ovarium we find a corpus luteum, presenting the following characters:—An oval body, with its longer axis varying from four to five-eighths of an inch; its shorter, three to four-eighths; its texture glandular, very vascular, the vessels running from its circumference to its centre; its colour, a dull yellow; its centre, hollow, or displaying a cicatrix. Such is a corpus luteum, or vesicle, which the ovum leaves behind it after it escapes from the ovary.

It is unnecessary for me to examine the question, whether the ovum be impregnated in the ovarium, or by a junction with the male semen after it passes into the fallopian tube? My opinion leans to the theory of ovarian conception, partly from the experiments of Dr. HAIGHTON, which demonstrated that evidences of generation may be produced in the ovaries, although from the closure of the fallopian tube previous to sexual intercourse, the semen be excluded; but chiefly from the evidence afforded by a case of ovarian gestation which I had an opportunity of watching, and the dissection of which is detailed by Dr. GRANVILLE, and illustrated by drawings by Mr. BAUER, in the *Philosophical Transactions* for 1820. "In this case, it was evident that conception had taken place in the ovarium, for the fœtus, which was as perfect as a fœtus in utero is at four months, was contained in one of the outer and inferior cavities of the ovarium; and the fragments of the corpus luteum, together with the placenta, adhered firmly by means of short filaments to the inner parietes of the cavity, within which conception had taken place." Be this theory correct or not, the question more immediately before us is the state of the ovarium produced by conception, and whether the presence of a corpus luteum is an infallible sign of pregnancy. This is, perhaps, the most prevailing opinion. In the trial of Mr. ANGUS for poisoning Miss BURNS, at Liverpool, in 1808, Dr. CARSON's opinion that she was not pregnant was thought to be irrefragably answered by the fact, which was not discovered until after the trial, that a corpus luteum was present in one of the ovaries. Now, a question arises,—does a corpus luteum ever occur in virgins, or independently altogether of coition?

In the museum of Dr. MACKINTOSH, of Edinburgh, are the ovaria of a child of five years of age, in whom the hymen was intact, in which are several, I think four, corpora lutea, as distinct, it is said, as in the impregnated female, differing only in their magnitude. But, notwithstanding this authority, I do not believe that one true corpus luteum can be found in the virgin ovary; every yellow spot is not a corpus luteum. In the ovaria of several women who had died virgins, and in whom the hymen was found entire, Sir E. HOME discovered what he regarded as distinct corpora lutea, and cicatrixes, which he supposes to have been left by ova that had passed out at some former period; but it must be allowed that Sir EVERARD HOME had a theory to support, his opinion being that the corpus luteum is not the result of conception, but of a state which renders the ovary fit to be impregnated, a commencing provision for a future conception. BLUMENBACH, also, thought that corpora lutea were found in virgins, but he does not affirm that he ever saw one himself, for he merely reasons respecting their existence on the authority of others. CUVIER, also, states that he perceived cicatrixes in the ovaria of women who had never had sexual intercourse. In Dr. BLUNDELL's possession, at the time he wrote his excellent paper on the Physiology of Generation, printed in the tenth vol. of the *Medico-Chirurgical Transactions*, were "the ovaries of a young girl that died of chorea, under seventeen years of age, with the hymen, which nearly closed the entrance to the vagina, unbroken. In these ovaries, the corpora lutea," he says, "are no fewer than four." "Two are very distinct, and differ from the corpus luteum of impregnation merely from their diminutive size, and the less extensive vascularity of the contiguous parts of the ovary." The inference that might be drawn from these facts is, that no confident opinion of the existence of impregnation can be founded on the discovery of corpora lutea in the ovaries on dissection; and this is decidedly the opinion of Dr. BLUNDELL, who, nevertheless, maintains the necessity of the semen having access to the ovum for completing the generative process. Notwithstanding my opinion, it must be admitted, that if we consider the origin of the corpus luteum, and the general idea that it is the calyx, from which the impregnated ovum has dropped, it is highly probable that the vascularity which constitutes this calyx a corpus luteum is a preparation for impregnation, or that state which fits the ovum for impregnation; that the orgasm of the sexual congress greatly increases this vascularity, and that the very distinct character of the corpus luteum which is seen immediately after conception, is probably produced at the time it takes place; and that the rupture of the vascular calyx, in the escape of the ovum into the fallopian tube, is the cause of its remaining nearly the same during the progress of pregnancy, and disappearing afterwards, from the powerful action of the absorbents which follows that process. This statement is not contrary to my opinion, supported by the conclusion of Dr. MONTGOMERY, of Dublin, author of a paper on the Signs of Pregnancy in the *Cyclopædia*

of Practical Medicine, namely, that the corpus luteum is the result of conception, and of nothing but conception. HALLER, also, asserts that he never met with a corpus luteum except in women who died pregnant,—"corpus luteum in virginiis animalibus nullum est, ex conceptione critur, neque prius paratum adest." Nevertheless, this author, and others who maintain the same opinion, admit that imperfect corpora lutea are found in virgins; but there is no external prominence over them, nor cicatrix; they are not vascular, and cannot be injected, and they have no central cavity.

My own opinion is in favour of the prior existence, but not the vascularity of this body, and although I maintain that the presence of a corpus luteum, in the simple signification of the term, is not an infallible sign of impregnation, yet I accord with the observation of Mr. STANLEY, that the corpus luteum, in an impregnated woman, is readily distinguished by its greater size from those bodies that resemble it in a virgin; and I would add, that the cavity of the corpus luteum in the impregnated female is always distended, or has a cicatrix over it; whereas in the virgin it contains the graafian vesicle, and is devoid of any cicatrix. Upon the whole, therefore, although I believe that the germ of the corpus luteum exists in the unimpregnated female, yet I accord with HALLER, that the real corpus luteum "is always the effect of conception." When substances such as Dr. MACKINTOSH'S specimen exhibits are found in virgins, we generally find that a tubercular diathesis has prevailed, that the yellow bodies are not vascular, but generally fibro-cellular, and that they present neither prominences nor cicatrixes on the surfaces of the ovaria. Such is the state of the ovarium. Now, what is that of the uterus in this early stage of pregnancy?

The first change which takes place in the uterus is the formation of the membrana decidua, a flocculent, yellowish, albuminous substance, which is developed a short time after coition, and lines the whole interior of the cavity of the uterus, separating, to a greater or less extent, on the entrance of the ovum into the womb, and applying itself around it in a manner somewhat similar to that in which the peritoneum envelopes the intestines. It pre-exists to the entrance of the ovum into the uterus, which usually occurs about the second week, and the appearance of which is a sufficient demonstration that pregnancy has existed; but if no ovum be found, still, proof may be obtained from the combination of the signs indicating pregnancy, as, for instance, the fulness of the mammæ, the darkening and change in the surface of the areola of the nipples, the large size of the corpus luteum, and the presence of the membrana decidua. Still, however, we must bear in remembrance that this membrane exists in what are termed false conceptions, and, therefore, that its presence does not always indicate that of an ovum and fœtus; but it never occurs without the sexual congress having taken place.

How are we to look for the fœtus at this early period? After having separated the uterus from the pelvic cavity, it must be laid softly upon a clean cloth, several times doubled, and wiped free from all blood; a sound, or director, is then to be gently introduced through the vaginal orifice into the cavity of the organ; and an incision having been made on the anterior of its surface, the sound will serve as a director to a bistoury, in slitting up the remainder, and at the same time avoiding its contents. The instrument is next to be changed, so as to enable you to make a transverse incision, in the direction of its fundus, or superior margin. The separation of the deciduous membrane must next be effected with the greatest care, as it is easily lacerated.

We are now to look for the ovum at the fundus of the uterus, behind the deciduous membrane, where we shall perceive it as a small vesicle, surrounded by a vascular web. If it be not in this place we shall find it either arrested in the fallopian tube, or remaining in the ovarium; as, in either case, or in any other of extra-uterine gestation, the deciduous membrane appears in the uterus, although no ovum be there. The ovum, when in the uterus, as I have already stated, is enveloped by the deciduous membrane, forming the membrana reflexa; the ovum is a soft, oval body, composed of two distinct membranes, besides the decidua, the chorion, and the amnion. The first at this period is flocculent and vascular on the outer surface, these flocculi serving to attach it to the uterine decidua, and being the rudiments of the future placenta; the second is the amnion, or vesicle, which at this time directly involves the fœtus; it is thin and pellucid, and contains a watery fluid, in which the fœtus is suspended. At this period the fœtus is an opaque oval body, not larger than a small cheese maggot, and closely attached to the amnion. In a short time afterwards it recedes, as the amnion enlarges, and the umbilical cord then shows itself. On dissecting carefully we find the placenta, which is formed by the vessels of the chorion, accumulated at that part where the ovum first adheres to the parietes of the uterus; this cake of vessels, therefore, is found to be double, the uterine portion of it being derived from the decidua, the fœtal portion being the vessels of the chorion, which are collected into the trunks forming the umbilical cord. The placenta is, in general, to be looked for in the fundus of the uterus, but sometimes it is attached to other parts, and occasionally over the orifice. Such are the signs which pregnancy within the first month, presents on post-mortem

examination of the uterus and ovaries; and if they be all present, and the uterus itself be swelled and softened in its substance, we can have no hesitation in pronouncing a decided opinion as to the existence of pregnancy at the time of the death of the individual.

2. With respect to the second head of inquiry, what are the appearances displayed by dissection in each succeeding month? As pregnancy advances the signs become less and less equivocal, and the medical jurist is merely called upon to decide upon the period of the pregnancy from the appearances of the uterus and its contents. The uterus undergoes important changes in size, figure, texture, and situation.

With regard to *size* and *figure*, within the first three months the increase of size is very slow; and although the fundus becomes more convex and broader, yet the figure is not very different from that of the unimpregnated uterus; on cutting into it, however, towards the termination of the third month, we find the small triangular cavity of the unimpregnated organ enlarged, and accommodating itself to the subglobular form of the ovum. The ovum itself, at the end of the first six weeks, is of about the size of a pigeon's egg; the decidua are distinct from one another, and the amnion from the chorion. At the end of the third month the ovum is as large as a goose's egg; the decidua are not separated, although still distinct; the amnion has enlarged, so as to approach the chorion; and the quantity of fluid contained in it is very large in proportion to the size of the fœtus, which is now seen suspended, with the head depending.

After the *fourth* month the original figure of the uterus disappears, and it acquires an oval or nearly globular form; its cervix is shortening, and the whole organ is found raised above the brim of the pelvis. The period of this change of situation generally becomes sensible to the mother, and constitutes what is generally termed *quickening*. At this time you will find that the disproportion between the fluid within the amnion and the size of the fœtus is greatly lessened; the fœtus is less pendulous; the head is generally inclined on the chest; the back bent, and usually placed towards one side of the mother.

At the end of the *fifth* month the enlargement is sufficiently obvious through the integuments; the fundus of the uterus approaches to the umbilicus, and the fœtus not only is larger but more perfect.

About the *sixth* month the fundus uteri reaches the umbilicus, and continues to rise until the termination of the eighth month, when it approaches the scrobiculis cordis; and, at the same time, the cervix is obliterated, flattened, and attenuated.

After the *ninth* month the uterus begins to descend: it is now about eleven inches in length, and upwards of nine in breadth; the decidua appear as a mere net-work of short white fibres; the placenta is usually about nine inches in diameter; the umbilical cord about eighteen inches in length; and the fœtus, which is now ready to sustain an independent existence, is about twenty inches in length, and weighs about seven pounds; but you will readily conceive that these proportions must vary according to circumstances, and therefore cannot be relied upon in a medico-legal inquiry.

Such are the appearances to be looked for, if the female die during the progress of pregnancy. But an abortion, or premature delivery, may be effected, or she may die immediately after parturition at the full period, and the fact of her delivery become a subject of medico-legal inquiry.

What are the appearances that indicate, in the dead subject, that delivery had recently taken place? In the very early periods, it is difficult to ascertain the fact of abortion having occurred immediately before death, from the post-mortem examination of the body. If, in a suspected case of this kind, the uterus is found of its natural small size, without any decidua in the cavity, we can have no stronger proof of the erroneous character of the charge: we can have no hesitation in pronouncing that abortion had not taken place immediately before death. It must, however, be admitted, that the ovum may be expelled entire before the placenta be firmly attached to the uterus; in which case it leaves behind it no token of its existence, nor does the state of the uterine cavity afford any certain indication of the organ having taken on the action which conception induces. After a few months, however, have passed, the uterus will be found larger and thicker than usual, its blood vessels and lymphatics greatly augmented in size, and the spot where the placenta was attached is indicated by the roughness of the surface; at the same time, the broad ligaments are partially applied as an extended covering to the enlarged uterus, whilst the round ligaments are relaxed. But these appearances, except the placental mark, may have been the result of hydatids; it is, therefore, necessary to examine the ovaria, and find whether a corpus luteum exist, with the scar produced by the escape of an ovum, and whether there be the usual enlargement of the uterine vessels attendant on pregnancy. If the delivery have been at the full period, the evidences are the following, which have been clearly laid down by Professor Burns, in his *Elements of Midwifery*:—The uterus is found like a large flattened pouch, from nine to twelve inches long; its mouth is generally sufficiently open to admit the hand to pass into it; the parietes are soft and relaxed; the cavity contains coagula, or a

bloody fluid, and the surface of the organ is covered by the pulpy remains of the decidua. Often, marks of the attachment of the placenta are very visible. This part is of a dark colour, appearing as if denuded, and displaying semilunar and apparently valvular openings in its structure, so that the uterus might be thought to be gangrenous by those who are not aware of the circumstance. The surface matter being elevated, the sound substance of the womb is seen; the vessels are extremely large and numerous; the fallopian tubes, round ligaments, and surface of the ovaria, are so vascular, that they have a purple colour. The spot where the ovum escaped is more vascular than the rest of the ovarian surface. This state of the uterine appendages continues until the womb has returned to the usual condition of its unimpregnated state. This condition of parts, however, rapidly alters, according to the time which the woman survives after delivery, affording more or less chance for the contraction of the uterus. The effect of time is also modified by the degree of energy of the habit of the woman, and the consequent contractile force of the organ; so that, in some instances, it may be as much contracted in a few days as after the lapse of a week. In general, however, at the full period of gestation it will be found about seven inches long and four broad, and its substance an inch, or one inch and a half, thick, the cut surfaces displaying a muscular aspect and the orifices of many large vessels. About a week after delivery, the organ is as large as two fists; at the end of a fortnight, it is five inches long, generally lying obliquely on one side; the inner surface is still bloody, and covered with a pulpy substance, like decidua. The muscularity is also still distinct, but its thickening is diminished one-third; the orbicular direction of the fibres round the orifices of the tubes are, however, still very evident; the substance is whitish. The intestines have not yet assumed the same order as usual; but the distended cœcum is often more prominent than the rest. It is a month, at least, before the uterus returns to its natural state; still the os uteri rarely, if ever, closes to the same degree as in the virgin state. It is only, therefore, within this period that any conclusions can be deduced from the appearance of the uterus; and even then no very decisive opinion can be formed, in many instances, unless the examination take place within the first ten days. Dr. Montgomery mentions a case, in which the os and cervix uteri scarcely differed from their natural unimpregnated state at the end of five days. In early abortions, little satisfactory information is obtained from an examination a day or two after the occurrence.

In the case of Miss Burns, already alluded to, in which a premature labour was supposed to have been effected by means of savine, all those appearances which indicate delivery were observed on opening the body after death; the vagina was still dilated, the labia were livid, and every where stained with blood; yet, as you have heard, a most scientific and able physician contended that these appearances did not indicate recent delivery. His arguments, in the first place, were, that the flaccid state of the uterus and the separation of the placenta must have caused an hæmorrhage sufficient to have gorged the uterus with coagulated blood, which was not the case; but Sir Charles Clarke stated, that he had seen cases of the uterus not contracting after delivery less than that of Miss Burns, and yet no hæmorrhage occur. Dr. Carson also contended that hydatids might have been the cause of the distension; but he forgot that the peduncle of a bunch of hydatids never can leave such a mark as the placenta left in this case; the mark left by the peduncle of hydatids, in no instance, has exceeded two inches in diameter, whilst that caused by the placenta is seldom less than nine inches. Dr. Carson also forgot that the delivery of hydatids constitutes a proof of previous sexual intercourse. The prisoner was acquitted on Dr. Carson's evidence chiefly; but few doubts remained in the minds of every other medical man who heard the trial, or read the report of it, that this unfortunate woman had really been recently delivered of a child; and this opinion was thought to be completely verified, by the discovery of a corpus luteum in the ovarium, on a second examination of the body after the trial.

The late Mr. Hey, of Leeds, brought the uterus and its appendages to London, and showed them to Dr. Denman, Dr. Haighton, Sir Charles Clarke, Sir A. Cooper, and Mr. Abernethy, all of whom concurred in the opinion that they indicated delivery at an advanced period of pregnancy; and they moreover subscribed to the opinion, that this fact was placed beyond a doubt by the discovery of the corpus luteum.

Upon the whole, the evidences of pregnancy, from post-mortem examination, must be regarded as equivocal in the very early stage; and less and less so after the second month. The signs of delivery are also equivocal in the early periods; but they are greatly to be depended on when the placental mark is very obvious; and they are perfectly unequivocal when the placenta is found, or has been retained in the uterus. This, however, is rare, and if the other signs be present, the placental mark may be regarded as one of the least equivocal indications of recent delivery in advanced pregnancy.

On a review of the facts and arguments that have been brought before you, we may conclude,—

1. That the evidence respecting early pregnancy, derived from post-mortem examination, is very far from satisfactory; but conjectures in confirmation of moral and other evidence may be obtained from the following observations:—

a. Almost immediately after conception, if the ovary be examined, a graafian vesicle will be found more than usually turgid, and on cutting into it the internal membrane will be seen somewhat thickened, corrugated, and more yellow than in the unimpregnated state. Professor Baer observed this in a woman who drowned herself the day after that on which she had had connection with her lover. This membrane is, also, in this early stage, detached from the external one, to which it firmly adheres in the unimpregnated state.

b. When the alantois is found we may conclude that the pregnancy had not reached the second month, as that appendage is never found in the human subject after the second month.

2. That after the second month the value of the evidence afforded by post-mortem examination is increased in the ratio of its proximity to the full period of uterine gestation.

3. That the evidence is most satisfactory when the examination has been made two or three days after delivery; but when the female has survived the delivery a week, then the signs become more equivocal, and after that period they begin to disappear.

4. A first delivery is more easily detected than any subsequent one.

5. In every instance the signs of delivery cannot be relied upon, unless taken collectively, and in conjunction, also, with other circumstances connected with the history of the case.

LECTURES
ON THE
PHYSIOLOGY OF THE NERVOUS SYSTEM,
DELIVERED IN 1836, IN THE COLLEGE OF FRANCE.

By M. MAGENDIE.

Lecture X.

The Cerebro-spinal Fluid, *(continued)*:—*Nervous Disorders of Insane Persons. Chemical Investigations of the Brain required. Ramollissement of the Brain; nothing of value indicated by the term. Apoplexy. The points in these diseases which really demand elucidation. Co-existence of Serous and Sanguineous Apoplexy. Nervous Apoplexy. More Inquiry demanded to establish the existence of Cerebral Fever; Transformation, in that disease, of the Cerebro-spinal Fluid into a solid.—The Cerebro-spinal Fluid in Birds, Reptiles, Batrachiæ, and Fishes.*

Gentlemen:—In my last lecture I drew your attention to some of the causes which modify the conditions of the nervous system, and showed how age was one of the most remarkable; the spinal marrow of an old woman, 85 years of age, presented us with an excellent example of this influence. I also mentioned some additional uses which the cephalo-rachidian fluid may be considered as fulfilling in the economy, and explained how it serves to envelop, and, as it were, isolate, the filaments composing the roots of the cerebral and spinal nerves. Finally, I pointed out another use, that of obviating the effects of friction against the osseous parietes, to which the nervous centres are exposed during the various movements which they execute.

At the close of my last lecture I entered on the consideration of a disease, interesting and important, in several points of view. The paralysis with which deranged persons are so frequently attacked is a phenomenon worthy of being studied with the greatest attention, for its mode of development throws light on many other affections of the nervous system, while the pathological results and symptoms are enchained together in the most evident manner. During life we observe an almost perfect annihilation of all the faculties and functions; after death what do we find? A great portion of the brain has disappeared by atrophy, and its place is supplied by an augmentation of the fluid.

The study of the nervous system of these paralytic persons would furnish matter at once curious and useful. I am convinced, if the brain were submitted to a minute and rigorous chemical analysis, we should obtain much more precise results than we have at present, not only on the nature of the cerebral substance, but what is still more important, on cerebral diseases in general. Hitherto the minds of physicians seem exclusively directed towards speculations, all of them more or less ingenious, instead of endeavouring to ascertain material facts as a foundation for their conclusions. To proclaim the brain a collection of organs, each of which has a special faculty, or mode of action, attached to it, to pretend to point out the exact limit of the cerebral substance in which each faculty resides, are hypotheses that can never advance the science of medicine a single step. But enough of this.

To complete what we had to say respecting the application of the physiological modifications of the cerebro-spinal fluid to cerebral disease, I shall now speak a few words on ramollissement of the brain, a term which is illchosen, insignificant, and little worthy of being received into a scientific nomenclature.

An individual presents some trouble of the intellectual faculties, some modification of sensibility in one of his limbs; the limb

now becomes weaker, is partially paralyzed, and begins to contract; this continues for some time, when he is, perhaps, seized with an attack resembling apoplexy. Coma and general paralysis set in; the man dies. The physician who examined his brain tells you a portion of the cerebral substance is ramollised, softened: thank him for his information. But what more does this teach than any one could have found out for himself, viz., that one part of the brain is peculiarly soft and pulpy? What relation does he establish between this physical phenomenon of change of consistency and the dreadful modification which the most elevated of our faculties undergo during the disease? How far better and more philosophical to try to ascertain whether the nervous matter of the brain has disappeared under this softening process, or what portion of it! How much superior to determine the exact nature of the modification, without thinking every thing complete when we have pronounced the word "ramollissement!" How much more practical our occupation if we examined the state of the fluid in these cases, and acquired precise notions on the changes it may have undergone! There are various other affections of the brain to which investigations of this kind are applicable. A child, for example, is attacked with chronic hydrocephalus; the cranium enlarges, while the rest of the body falls away; convulsions seize the child, or it falls into a state of coma, and dies. On examining the brain we find the ventricles distended with an enormous quantity of fluid; but this is not all, the central parts of the brain are profoundly modified; the septum lucidum has disappeared altogether, with, perhaps, the fornix, and a portion of the thalami, or corpus callosum; all these parts seem as if they had been dissolved in the fluid, which, itself, is more or less modified, sometimes clear, but often turbid, mixed with blood, flocci, or purulent matter. All these facts demand more attention than has been bestowed on them, but attention of a different kind; we should not confine ourselves to merely noting the changes before us; to saying, this is soft, this firm, this injected, that normal. We should be aware that the cerebral tissue is dissolved, and suspended in the fluid. We should seek to know why these particular portions of the brain are subject to a modification which does not manifest itself in any other part of the nervous system; why the septum lucidum, in a normal state, remains unchanged in a fluid at 30° R.; and why, under other circumstances, it is changed into a fluid pulp when exposed to the action of the morbidly-produced cerebro-spinal fluid. Let us pursue these considerations a little farther.

There are certain diseases which give rise to a rapid augmentation of the fluid, the latter remaining either unchanged in its appearance, or being altered in composition and qualities, through the effect of the disease. This is chiefly seen in acute affections of the brain, and naturally leads us to speak of "serous apoplexy." Does there, or does there not, exist a disease called "serous apoplexy?" MORGAGNI, that great authority in matters of pathological anatomy, did not hesitate to describe this disease as a considerable effusion of serum into the ventricles of the brain, independently of every other lesion. Now-a-days pathologists will tell you, that such an affection is imaginary, and cannot exist, or pass over altogether the ideas of their predecessors, without noticing them. I would willingly abandon the term "apoplexy," a term which is evidently unsuitable, which bears no relation whatever to the cause or nature of the disease, for, as the etymology indicates, it merely refers to one of the circumstances under which the disease is developed, namely, that of striking suddenly, (besides, this phenomena is by no means a constant one), and I would adopt in its place any word which expresses accumulation of fluid. However this may be, if we would really determine whether an individual has died from a sudden effusion of serous fluid into the ventricles and subarachnoid tissue, we must proceed with our autopsies in a manner quite different from the one we are accustomed to employ at the present day, when you will find our internes opening daily two or three bodies in an hour or an hour and a half. This is too quick work; we must lay aside the smith-like blows of the hammer, and the not less destructive, though more delicate, evolutions of the scalpel, which we are now in the habit of employing,—an excellent method, indeed, for breaking down every obstacle, but the worst possible for investigating the anatomy or pathological changes of so tender an organ as the brain. We should conduct our autopsies as they were conducted fifty years ago—as the immortal MORGAGNI conducted them.

Before presuming to pronounce on the existence of a serous apoplexy, we should be well acquainted with all the phenomena presenting themselves during the course of the hæmorrhagic species. When at *Salpêtrière* I had frequent opportunities of observing all the symptoms of hæmorrhagic apoplexy, produced by a rapid augmentation of the cephalo-rachidian fluid. Guided by a habit of observing, and pushed on by the desire of ascertaining the exact seat of the lesion, I found in many instances that the disease had not consisted in an effusion of blood into the substance of the brain, as the symptoms seemed to indicate, but in a rapid accumulation of two, three, or four ounces of fluid in the cerebral ventricles. Thus I was enabled to establish that "serous apoplexy," (which it would be better to call "rapid augmentation of the

cerebro-spinal fluid"), did as certainly exist as hæmorrhagic apoplexy, though it was a much less frequent disease. I have seen one apoplectic stroke soon followed by a second; and after death we found a collection of blood in one ventricle, and in the other a collection of serous fluid. The quantity of blood effused amounted to several ounces; that of the serous effusion was still more considerable. Thus you see serous and sanguineous apoplexy may co-exist in the same individual.

As to the disease called "nervous apoplexy," I will not say that it cannot exist. It is possible for some internal cause to cause sudden derangement of the nervous functions, and even destroy these latter, without leaving behind any material lesions in the brain or in its appendages; in the same way that simple concussion from a blow in the head may kill one suddenly; or the contact of an atom of prussic acid, suspended in the atmosphere, may suddenly suspend the nervous influence. But we should make a most careful investigation of the brain, its membranes and cavities, before we pronounce a disease "nervous apoplexy," and say that death has been induced without any material lesions. But let me repeat once more, for this we require the utmost care in examining the body, and no hammer-strokes. The fact is, however we may boast of the advancement of our science in these latter times, that an immensity yet remains to be done in diseases of the nervous system. What precious observations might have been made on the brains of so many illustrious individuals lately cut off in this country and in England! Yet how much has been neglected! We have acquired no positive knowledge on the weight, the conformation, or the structure of their nervous systems, yet how many points worthy of the most serious reflection would a careful dissection of their intellectual organs have furnished us with! Not long ago I had the misfortune to lose an illustrious and learned friend, whose care and kindness for me equalled that of a father. I examined his brain with the utmost minuteness, and in future years I hope to publish a detailed history of the phenomena which I observed, and many of which have been already pourtrayed in the work of Dr. CARSWELL of London.

You may find an augmentation of the fluid not only in chronic affections, but also an increase sufficient to produce fatal consequences in many acute fevers. In cerebral fever, properly so called,—phrensia of the ancient writers—ataxo-adynamic fever of some modern authors,—the state of excitement in which the patient lies evidently points out an augmentation of the fluid, and this is proved by the autopsy, when we discover more or less serous fluid at the base of the brain, or in the hemispheres. However, I should remark to you, that this augmentation alone does not always induce death; while, on the contrary, life is incompatible with any change in the qualities or nature of the fluid.

When, in the cerebral fever of children, the intelligence is gravely modified, the little patient at length being unable to recognize those who surround him, or answer any of the questions which his friends or parents may address, in such cases we may say there is not only augmentation, but alteration of the fluid; the modification of the one manifests itself by the trouble it occasions in the functions of the other. In some rare cases this modification is carried so far, that a greater or less portion of the liquid is transformed into a solid matter; and in such case you can readily conceive how the action of the nervous substance in the extremities of the nerves must be impeded, or altogether prevented. I remember seeing, at the Hôtel-Dieu, a case of inflammation of the pia mater, extending from the cavity of the cranium down to the sacrum. Instead of the cerebro-spinal fluid, there extended a layer of solidified matter all over the cerebro-spinal axis, and, accordingly, the whole functions of the nervous system were reduced to a nullity. Let us, then, lay it down as a general rule, that every time that the qualities of the cerebro-spinal fluid are troubled, the functions of the nervous system are troubled.

Such are the principle facts that we were desirous of laying before you relative to this fluid; it is in this manner, and towards something positive, that our studies should be directed, if we wish to make any real progress in the sciences. Cast aside your old prejudices and ideas,—for the most part expressive of the ignorance of which people speak of the existence of this fluid; and now that you are acquainted with it, examine every circumstance; weigh well every consideration connected with it; observe constantly, and without prejudice; and you cannot fail to arrive at happy results.

I have already mentioned, in a former lecture, that man was not the only animal in whom the existence of the cerebro-spinal fluid had been demonstrated; the fact is, we find it in all classes of vertebrated animals, in more or less quantity. Thus, in the class "birds" we find but a very small quantity of fluid on the surface of the cerebrum and cerebellum; very little near the optic tubercles; indeed, we do not find exactly a collection of fluid, properly so called, but the surface of the brain and its cavities are moistened with a thin layer of liquid matter, laid on in too small a quantity to produce any discharge of fluid, as in the mammalia, when the cavity has been opened. The reason of this depends on the medium in which the animal is destined to live. In

birds, as you know, every thing is arranged for lightness, and the faculty of mounting rapidly in the air might have been impeded by any considerable collection of fluid in the head. However, though there is no liquid, properly so called, we find a disposition for the protection of the brain, which consists in a vesiculated osseous tissue, placed between the two plates of the cranial parietes; and hence, although a bird's head may appear very large, it is very light in proportion to its volume, for the intervals of the tissue now mentioned are filled with nothing but air. In the bird tribe, however, the cerebro-spinal axis presents two points, one in the cerebellum, near the termination of the calamus scriptorius, at the junction of the head and neck, in which the fluid matter is laid down in greater abundance. Is it because greater mobility is requisite in this latter situation? Probably enough. The other point is in the lumbar ventricle, where we observe an excavation, or kind of furrow, on the median line, which contains a good deal of fluid. However, in this part of the body we cannot discover any connection whatever between the mobility of the parts and the accumulation of the fluid, unless we bring into account the ascent of the spinal marrow in totality. Hence the presence of fluid at these two particular parts depends on some purpose with which we are as yet unacquainted. Generally speaking, these birds have hardly any fluid. Look at the cock; the spinal marrow fills almost entirely the cavity of the rachis; yet the existence of a fluid matter here is certain; it not only clothes the surface of the nervous substance, but enters into the interior.

In reptiles and batrachiæ we find a certain quantity of the fluid in the skull; when the head of a frog is opened, for example, we collect some between the brain and its general cavity; however, I cannot affirm whether or not reptiles possess an arachnoid membrane, or whether the fluid is contained in the sub-arachnoid cellular tissue, or elsewhere; to determine this we should dissect some large reptile, such as the tortoise, &c. However this fact may be decided, the surface of the brain and spinal marrow in reptiles is covered with a moist layer of fluid, though it does not flow away when opened, as in the mammalia.

In the different classes of fishes, we always find something which replaces the cerebro-spinal fluid. Look at this carp; see how small its brain is; the rest of the cavity is occupied by a mass of osseous cellules arranged in the most remarkable way, and something resembling dried pulmonary tissue. The cellules are not filled with æqueous fluid, but with a fatty matter, which in fishes occupies the immense interval left between the brain and parietes of the cranium. This fatty matter is lighter than water. When we dissect a living fish, we find, independently of this fatty substance, a cellular arrangement, which seems to replace the arachnoid in this class of animals, and is disposed between the dura mater and pia mater. The cellules contain a very slight serosity, and when removed, the cavity of the spinal marrow gives issue to a quantity of serous fluid, which represents the cephalo-rachidian fluid much more closely than the fatty matter before alluded to. Besides, as the nervous system of reptiles does not contain any ventricular cavities, we could not expect the analogy to go very far between them and vertebrated animals.

CLINICAL LECTURES AND REMARKS,

DELIVERED ON

DISEASES OF THE SKIN, VENEREAL DISEASES, AND *SURGICAL CASES*,

AT THE

SKIN INFIRMARY, AND AT THE JERVIS-STREET HOSPITAL, DUBLIN.

By Dr. WALLACE,

SURGEON TO THOSE INSTITUTIONS.

PUSTULAR GROUP OF VENEREAL ERUPTIONS:—*Third Case, to Illustrate the first Variety of this Group. Symptoms and Treatment of the Case. General Characters of the first Variety of Pustular Syphilis. Its Symptoms, Prognosis, Diagnosis, and Treatment with Mercury and the Iodide of Potassium. Authors who have alluded to this Form of Pustular Syphilis.—The Relationship of the Varieties of Venereal Eruptions.—Similarity of Treatment required by the severest Forms of the Pustular and by the severest Forms of the Exanthematic.*

GENTLEMEN:—The next case of venereal eruption to which I wish to direct your attention is that of H. P. It belongs to the same group as that which we reviewed in the two last lectures. The state of this patient when you first saw him is thus reported:—

"Aged forty years; a smith by trade. His tonsils and pharynx are ulcerated. He speaks in a whisper, his voice being nearly lost. His drink returns by his nose, and his hearing is much impaired. Several of his joints, particularly his knees, are swelled, and painful. There is a great number of cicatrixes scattered over the surface of his body. These are livid-coloured, and generally below the level of the skin. There are also, in different situations, several small crusts, and the surface under them

is ulcerated. There is one very large limpet-shaped crust on his left fore-arm, not less than an inch and a half in diameter, and fully an inch in projection. It is, like all such crusts, composed of concentric circles, among which there exist bean-like particles of cuticle. On removing this crust, the under surface of which is hollow or excavated, a large whitish ulcer is exposed, having a flat surface, a sharp and nibbled edge, and a perpendicular border. He is pallid, and extremely emaciated. His pulse is rapid and feeble, and he has copious night perspirations."

What was the early history of these symptoms? Here I can afford you an illustration of the advantage of registering carefully the diseases of dispensary patients, for this man had been at two previous periods under my care, and I preserved notes of his symptoms on both occasions.

It is some months since he first came under my observation, and I find from my case-book that he then had "a rounded, whitish, excavated ulcer on the upper surface of the glans penis, the size of a split-pea; and on the same part, in the immediate vicinity of the ulcer, a pustule, the size of a grain of duck-shot, having a tumid red base and yellow apex." He described the ulcer as having commenced a few days previously, in the manner in which the pustule then appeared. In short, he had two stages of the common form of pustular primary syphilis, well marked,—the stage of pustule, and the stage of ulceration. The nitrate of silver was applied to both the ulcer and the pustule, so as to produce an eschar; the apex having been previously removed from the pustule. Both spots were almost healed in the course of a week, and he ceased to attend, just when I was about to commence, for the protection of his constitution, mercurial treatment. Of him I heard nothing more for three months; he then applied, labouring under an eruption, of which I have preserved this drawing, and a report, which I shall now read to you. You will observe all the appearances noted, well represented in the drawing.

"The spots are very numerous; some of them are on a level with the surface, of a livid-red colour, and of the diameter of a small pea; others are a little larger, of the same colour, slightly elevated, and scaly. Others are still more elevated, and pustular, or present a white centre, surrounded by a livid-red areola. Others have, in the centre of a similar areola, yellow-brown scabs, surrounded by a white line. The crusts on some of these spots are sunk, and their areola is scaly; on others the crusts are slightly elevated. There are several ulcers; these are circular, their edges sharp, their margins perpendicular, surrounded, like the other spots, by a livid-red areola, and their surfaces are pulpy and whitish, particularly at their circumference. The spots which are encrusted present, when the crusts are removed, similar ulcers. The eruption is thicker over the back and forehead than in any other situation. There are only a few spots on the lower limbs, and these appear, in general, to be dying or fading. On the scalp there are also a few, and when their crusts are removed, ulcers, having the same appearance as those on other parts of the body, are exposed. There are, also, a few spots on the outer skin of the penis, on the scrotum, and in the angles between the scrotum and thighs, which present the same common characters. Complains of 'great scalding and burning soreness' in his skin, and compares the sensation to that which would be produced by hot iron. His sleep is wretchedly bad in consequence of the pain of the sores.

"He has a large ulcer on each tonsil, and a still larger one on the back of the pharynx. These ulcers nearly run into each other. The mucous membrane surrounding them is of a remarkably dusky or livid-red colour. Their edges are sharply cut, and their surfaces of an ash-yellow colour. Has very great pain in swallowing. He also complains of pain or soreness in the back part of his nares.

"There is a hardness under the left angle of the jaw, which he says is the remains of a lump that came at the same time as the sore throat. He has no pains in his joints or bones, nor does he complain of any internal disease. It is five weeks since his throat became sore, and three since the eruption commenced first on his forehead, and subsequently on his back. It was principally during the night that it appeared. It occupied three or four nights in coming out, and during these nights he had sweats so profuse as to oblige him to change his linen. For a week or two previously he felt remarkably indolent, and subsequently had great sickness and headach. The latter symptoms were much relieved on the appearance of the eruption.

"He informs me that the affection of his penis for which he had formerly been in attendance, healed a few days after I had last seen him; that he then considered himself well, and had apparently remained so until the symptoms which preceded the existing eruption commenced."

Gentlemen, if you connect the reports made of the patient's state at different times, and which I have now read to you, you will have a tolerable account of a strongly-marked case.

When he first appeared labouring under constitutional symptoms, there were several circumstances which attracted my attention, and upon which it is worth while to make a few remarks. Thus, I could not avoid noticing that the eruption was very severe, although the primary symptoms had been

very mild. The eruption was also remarkable for the extent of surface which it covered; for the rapidity with which it had come out; for the deep livid colour of the spots; for the quickness with which ulcers were formed, for in the course of a few days the whole surface of his back and forehead presented an appearance as if it had been bored at distances of an inch with the end of a hot iron. I had also occasion to note that the redness which surrounded the ulcers in his throat was like that of the spots on his skin, remarkably livid. This tint uniformly argues a bad form of disease.

He did not employ, so long as he was under my care on this occasion, any remedies except such as were suited to keep down fever and irritation; and I believe it was discontent at my not putting him at once under the action of mercury that caused him to absent himself. He, however, soon found another who satisfied him in this particular, and rendered him the miserable object he presented when you first saw him. His was, at its outset, a case which required the greatest caution. It was one which mercury, judiciously employed, would have cured, but it was one in which that medicine would have been apt to go astray, and do, as you see it did do, a great deal of mischief.

What was the treatment adopted by us when he returned in the reduced state described in the report? The further use of mercury was forbidden. He was in a state of mercurial cachexia, that is, although under the influence of mercurial action, his venereal symptoms were either stationary or becoming worse. In such a state, a further employment of mercury always encreases the mischief. The day, however, once was, when such a state was the signal for encreasing the dose of the specific; and hence the sad consequences formerly so frequent. Such practice is no longer to be heard of, even among the most ignorant. Now-a-days, cases like the present are generally consigned to sarsaparilla, nitrous acid, and country air, and good practice it is compared with the old practice. It is, however, little better than an example of what is called by the French "médecine expectante." It is a practice which could not bring about a favourable termination for months, and even at the end of that time we should most probably be under the necessity of having recourse to mercury to wind up the cure. It is, Gentlemen, for the treatment of such cases as the present, and of some others, that modern surgery has been greatly enriched by the introduction of the hydriodate of potash, as an anti-syphilitic. You saw the rapidity with which this medicine acted, without the assistance of any others. I need not occupy your time with reading the reports. You can examine them at your leisure. In the course of three weeks, not a single bad or unpleasant symptom intervening, this patient's complaints were all removed, his strength restored, his flesh, his spirits, his appetite recovered. His medicine was, however, continued for three weeks longer; and when he was discharged his recovery seemed to be perfect. Since then I have heard nothing of him.

Let us now compare the three cases which we have reviewed in the present and two preceding lectures, and therefrom deduce

The General Characters of the First Variety of Pustular Syphilis,

or, of that form of venereal eruption which I have selected them to illustrate. In other words, let us consider the points in which these cases resembled, and those in which they differed, from each other.

1st. The eruption was, in all, purely pustular; and in two the primary symptoms were also, without doubt, pustular, while there was good reason for supposing that the same symptom was pustular likewise in the third.

2nd. The pustules were, in all the cases, of a very large size; many of them formed ulcers of great magnitude, several of which were covered with the true limpet-shaped crusts; and the ulcers singularly resembled each other in their mode of extension and their mode of healing.

3rd. The throat and mouth were, in all the patients, severely ulcerated.

4th. The synovial and fibrous systems suffered much in the first and last case, but scarcely at all in the second; and the degree in which those parts were affected in the same case at different times was inversely to that of the cutaneous system.

5th. The exfoliations of the alveolary processes and of the bones of the nose occurred in two patients, in consequence of the ulceration of the corresponding mucous surfaces; but, with this exception, the osseous system did not seem, in any of the cases, to be much affected.

6th. In all, the symptoms of syphilitic hectic were exquisitely marked.

If any form of venereal disease exists, which, if left to itself, would cause death, I have no doubt but that the present is one. I have seen two men who were reduced by it to a state of extreme exhaustion, who were, in fact, on the verge of death, and it was remarkable that both these patients were subsequently cured by very mild mercurial treatment. This fortunate result would not, perhaps, have taken place, had it not been that their complaints were not previously mismanaged by the use of mercury. In fact, they had not used any, for the nature of their disease had been so mistaken that it was not suspected to be venereal; otherwise, no doubt, this specific would have been administered in the common way, and thus their cases would, most

probably, have been rendered incurable by mercury.

The form of pustular syphilis which we have been considering, may be easily distinguished from the other forms of pustular venereal eruption, by those general characters in which, as I have said, the cases resembled each other, but particularly by the size of the pustules, by the size of the ulcers, and by the limpet-shaped crusts with which they are frequently covered. On the other hand, if you bear in mind the characters of a pustule as compared with an exantheme, you will never confound this eruption, which is pustular, with any of the exanthematic class. I shall, however, enlarge on its diagnosis in this point of view, when I speak of such varieties of the exanthematic tribe as it may be confounded with, particularly the bullous and tubercular varieties.

I know of no non-venereal diseases of the skin with which this form of pustular syphilis can be confounded, unless, perhaps, with those described by BATEMAN, by the names *rupia prominens* and *ecthyma cachecticum*. The rupia of BATEMAN is, however, bullous or vesicular, while the disease of which I am now speaking is pustular. On the other hand, the ulcers of ecthyma have no resemblance to those of venereal origin. To this we may add, that the symptoms which accompany and precede the venereal form of eruption will powerfully assist the diagnosis. I would also observe, *en passant*, that I am now speaking as if it were certain that there do exist such diseases, independently of the venereal poison, as those which Dr. BATEMAN has described and delineated under the names of rupia *prominens* and ecthyma *cachecticum*, for of this I have, after twenty years close attention to the subject of cutaneous diseases, very considerable doubts. Indeed, I have long been of opinion, as I have already more than once told you, that the diseases which have under these names been delineated by BATEMAN, as well as some of his varieties of lepra, are of venereal origin. I shall, however, have occasion to speak again on this topic at a more appropriate time.

I have now little to add to what I have already said on the subject of treatment. So far as I have yet seen, the hydriodate of potash is greatly to be preferred to mercury in this form of syphilis. Mercury will, however, as I have several times observed to you, if judiciously employed, effectually cure it, in a rapid and secure manner; but not by any means so rapidly, nor with so little trouble, as the hydriodate of potash. I would, therefore, under ordinary circumstances, abandon the constitutional use of mercury in this form of disease. Not so, however, its topical employment. In the cases we have considered, all topical applications which could influence the morbid actions, were intentionally avoided, that you might the better judge of the effects of the hydriodate of potash. This is not, however, the plan I would recommend you to pursue in practice, for you will be able to accelerate very much the cure of your patient by the topical use of mercurials. Of this class of remedies, you may employ the black wash, the yellow wash, the nitrated mercurial ointment, the common blue ointment, or mercurial fumigation; or you may alternate these with one another. Fumigation with cinnabar is valuable, where the mucous surfaces are diseased, and the more necessary as the hydriodate of potash is sometimes slow in acting on their ulcerations. I should observe, that you may commence the specific treatment of these cases without delay, or as soon as they fall under your care, for no preparation is required.

Now, although the three cases which we have reviewed may be considered as affording fair specimens of the first variety, or of the severest form, of pustular or ecthymatous syphilis, you will meet in practice examples of this disease which differ more or less from them: you will find such reported in the case-book. It is not, however, possible to devote, at present, so much time to the subject as would enable us to review them all; but it will, perhaps, be in my power to read them over at some future period. I shall then mark any peculiarities which may exist amongst them; and subsequently communicate to you my remarks.

I regret that I cannot direct you to any work where you will find the variety of venereal eruptions which we have been considering fully and accurately described. Some of its symptoms are detailed by TORELLA, by FALLOPIUS, by CULLERIER, by RAYER, and by SCHEDAL and CAZENOVE, as well as by several of the more modern writers in our own language. But by all these authors its description has been mixed up, more or less, with descriptions of other varieties of venereal eruption of the pustular, bullous, tubercular, and scaly kinds.

Were it true, as by some affirmed, that the different forms of venereal eruption may occur in the same individual, at the same time, or at different times, the result of the same infection, it would be of little use to draw a line of distinction amongst them. So far as my investigations have extended this is not, however, exactly the case. I will here repeat the results of my experience to the present moment on this topic. The true exanthematic forms never exist with the genuine pustular; but the different exanthematic forms may exist together, or in succession, after the same contamination; or, the different pustular forms may exist together, or at different times, the consequence of the same infection. It is, however, to be remembered, that the effects produced by the pustular virus approach nearer and nearer to the exanthematic character,

ceteris paribus, according as it has existed longer in the system. I believe you will find that the asserters of opinions contrary to these statements, speak of what they do not understand.

I admit that, in a practical point of view, there is at present little advantage in distinguishing the form of syphilis which we have been considering from the severest of the exanthematic forms, for, so far as we yet know, the severe forms of both tribes require the same mode of treatment. In fact, the same kind of constitution which gives rise to one of these forms will give rise to the other; that is, if the exanthematic virus would produce in any individual its worst type, as bullæ or tubercles, which cause large ulcers and projecting crusts, the pustular virus would produce in the same individual its worst type. You have seen cases which demonstrate these truths, and I shall hereafter review them. But the laws which regulate morbid actions, whether we do or do not see their practical application, should always be carefully noted, for we know not how soon they may be made available in practice; and the pathologist who disregards all considerations, the immediate practical tendency of which he cannot see, is little likely to extend the boundaries of science.

As time would not permit me to complete the review of any of those cases to which I propose next to direct your attention, I must postpone their consideration until we next meet.

REPLY TO A CRITICISM

ON A PAPER BY DR. ASHWELL, ON

CHLOROSIS AND ITS COMPLICATIONS.

To the Editor of THE LANCET.

SIR:—In the *British and Foreign Medical Review* for this month (January, 1837,) there is a critique on a paper of mine published in the 3rd part of the *Guy's Hospital Reports*, "On Chlorosis and its Complications." I am unwillingly induced to notice this for two reasons:—First, because Doctors Conolly and Forbes, its responsible editors, are highly respectable as medical translators, compilers, and reviewers; and, secondly, because the article is unjust, and savours more of personality than might have been expected from a strictly impartial critic.

I beg to premise, that I do not at all question the right to review or criticise any published production, nor am I disposed to scrutinize very rigidly the practical qualifications for such duties of critics, even though anonymous. High seasoning may be necessary in these works, piquancy of expression may even be desirable in talented articles of this kind, and occasional injustice, such, for instance, as that of which I now complain, may, possibly, be lost sight of, in the good effected by clever reviewing. An author, too, seldom suffers much now-a-days, from the unwarrantable personality of obscure and unknown writers: the public and the profession are well aware, that medical practitioners and reviewers, like other men, must strive to obtain pecuniary returns, if not from practice, which is the more difficult method, why then from reviewing, which is more easy, and they estimate accordingly, the praise or dispraise of stipendiary critics.

The reviewer states, "that I neither think clearly nor express my notions with simplicity and precision;" and this not being sufficiently personal, "I am also charged with verbosity, a want of lucid order, pages abounding with prettinesses;" and, finally, which is sufficiently malignant, the reviewer recommends students, for whom the essay was especially intended, "to avoid its perusal."

I ought, perhaps, in fairness to this diffident reviewer, to state, that notwithstanding all these defects, he perceives internal evidence in the paper "that Dr. Ashwell has seen much of the disease in question, and (with regard to treatment) is a sensible and judicious practitioner." Again, too, he willingly allows, "that there are some hints, particularly as to the danger of functional passing into organic disease, well worth attending to." "Some of the cases are also very instructive."

I have heard of reviewers who read only the table of contents, and having glanced superficially through the book, ask themselves, or their employers, whether they are to damn the work or to praise it? Whether or not such has been the case with my essay, I leave others to determine; but surely as to clearness and precision of expression, nothing, as it appears to me, can be more clever, or more precise, than the three positions, which it is the entire scope of the paper to illustrate:—

First, "That chlorosis, complicated with amenorrhœa, is the most common derangement of the catamenial function." Secondly, "That if such a complication continue long, it will produce functional disturbance, at least, of the nervous, digestive, vascular, and respiratory systems; and that where a fatal termination ensues, it will generally be in phthisis." And, Thirdly, "That the treatment to be successful must be early commenced, and most sedulously prosecuted."

A novice in medicine would easily have followed such an arrangement; but not so the reviewer; to *him*, it is perplexing and verbose. Still, he kindly, and complacently, agrees with the opinion on which the essay is founded, that chlorosis depends on a

morbid condition of the blood, and not on amenorrhœa; and, afterwards, with singular inconsistency, he censures, as confused and illogical, my attempt to trace to this morbid condition of the vital fluid, the various complications of the disease. A specimen of this reviewer's want of accuracy and practical knowledge is immediately furnished, in his erroneous statement, that those pains of the head denominated by me "complications of chlorosis with functional affection of the cerebrum," "are identical with the anomalous headachs of young women, regarded by Sydenham as forms of hysteria." My refutation of this critical mistake is easy. If Sydenham had intended to describe, as I did, the headachs connected only with chlorosis, he would have designated them chlorotic, not anomalous, headachs; and if the reviewer had carefully studied disease, instead of loosely reading Sydenham, *he* might have happened to know, that hysteria, considered apart from chlorosis, is not unfrequently a disease of repletion and congestion, and then, in reference to treatment, even *he* must have perceived the propriety of my distinction. A disease, like chlorosis, essentially characterized by anemia and pallor, when complicated with pain of the head, would be aggravated by depletory and antiphlogistic measures; while female patients, affected by the anomalous and hysterical headachs of Sydenham, as well as my reviewer, who, perhaps, may not suffer so much from headach as from stupor, or other disturbance in his cerebral functions, would, doubtless, be relieved by taking away blood. It were easy to enlarge on hysteria being merely a symptom of chlorosis, and disappearing when the chlorosis is formed; but enough, perhaps, has been advanced to neutralize this weak attempt of the reviewer to destroy the practical inductions of my paper. One other effort is made by this intelligent individual to discredit my pathology, and it is too curiously bookish and recondite to be silently passed over. I am censured for using so lax a term as "functional affections of the cerebrum;" and to show its want of point, this hypercritical writer informs his readers, that these painful affections may depend "not only on the cerebrum, but on the cerebellum, or membranes, or bones, or nerves supplying the parts above them." This certainly is *lucid*, but surely the reviewer ought, comprehensively, to have added, or from the heart, the lungs, the kidneys, or the great toes. I may, however, say, that these pains were attributed *to the cerebrum as a whole*, from the very difficulty, which the reviewer has so clumsily expressed, of precisely fixing on the part within the cranium especially affected; and I appeal to general and established usage, as justificatory of this nomenclature. Your readers will judge how candidly and ably the essay is examined, when I tell them, that its most important parts are untouched.

The quibbles already exposed comprise all in the review bearing the semblance of argument. The "complication of chlorosis with phthisis," a most important, and, I believe, a newly-explained form of the affection, is not, in the estimation of the critic, worth one observation. We cannot, however, expect, that the assumed knowledge of every contributor to a review should be founded on practical experience. Were this required, in many cases "Othello's occupation" would he gone. Surely I have deserved more courtesy at the hands of these learned gentlemen, especially after the impartiality and independence so loudly vaunted by them only a year ago, as all the papers which I have yet had the pleasure to contribute to the *Guy's Hospital Reports* have been new, demanding careful scrutiny, not vulgar abuse. The essay so summarily disposed of, was the result of the observation of several years, in hospital and private practice. I am quite easy to leave it in the hands of the profession, as, already, from not a few of its members, I have received more approbation than it probably deserves. The flippant and anonymous condemnation of the critic will not injure the essay, although I am not equally certain that this infantine journal, infantine, by-the-by, in other points than its age, will escape with equal impunity. I can easily conceive, that in the partial retirement of Chichester, and amidst the romantic scenery of Warwick, it may be much easier, and more pleasant, to cultivate theory and medical literature, than to toil, day after day, in public as well as in private practice, to investigate disease, and to give to the profession, through the medium of the lecture-room and the press, the results of such labours. Anonymous reviewing is certainly far easier. But these rural and Arcadian reviewers, these twin critics, Dr. Forbes and Dr. Conolly, should remember, that such light and elegant repasts will not satisfy professional curiosity, neither will such nectar allay the present thirst for sound practical knowledge; and as I really wish well to their little bantling (the *British and Foreign Medical Review*), I may be permitted, most respectfully, to assure them, that strong arguments and courteous language will increase the sale of the journal infinitely more than opinions upon professional subjects, mixed up with the publication of gratuitous or offensive personalities. I am, Sir, your obedient servant,

SAMUEL ASHWELL, M.D.
Obstetric Lecturer and Physician to Guy's Hospital

January 7th, 1837.

(Note.) As an instance of the loose analogies of anonymous reviewers, showing how little such writers are to be depended

on, I transcribe the passage from Sydenham, on the strength of which Messrs. Conolly and Forbes' critic has ignorantly assumed that my "functional affections of the cerebrum" are identical with Sydenham's hysterical headachs, whose pathology of them is in admirable keeping with the professional knowledge of my able and enlightened reviewer :—

"Sometimes this disease (hysteria) attacks the external part of the head, between the pericranium and the cranium, and occasions violent pain, which continues fixed in one place, not exceeding the breadth of the thumb, and it is likewise accompanied with very violent vomiting. I call this species the *clavus hystericus*." Vol. ii., page 108, of Dr. Wallis's Translation of Sydenham.

"To this irregular motion of the spirits the clavus hystericus is to be ascribed, in which all the spirits are collected in a certain point of the pericranium, occasioning such a kind of pain as if a nail were driven into the head, and attended with a copious discharge of green matter by vomit. And this contraction of all the spirits into a kind of point differs little from the collection of the rays of the sun by a burning-glass; for as these burn by their united force, so those for the same reason cause a pain, by tearing the membranes with united violence." Page 121, vol. ii.

MR. LEWIS'S METHOD OF TREATING HYDROCELE.

To the Editor of THE LANCET.

SIR :—In THE LANCET of the 7th of May last I pointed out to the medical profession a new method of treating hydrocele, *i. e.*, by puncturing with a very fine needle, until a drop of fluid oozes out on withdrawing the needle, and in three days the hydrocele will completely disappear, no matter in what quantity the fluid may have been collected. Since the publication of the above letter I have had numerous opportunities of testing the success of the new mode, and I am happy to say that, *out of upwards of 50 cases there has not been a single instance of failure*, neither has there been any consecutive inflammation, although some of the patients have travelled into the country immediately after the puncturation.

It may seem almost incredible that a single puncture with the point of a fine needle into an encysted cavity, is sufficient to cause the absorbtion of the fluid contained therein, but such is the fact. I have punctured an enormous double hydrocele, complicated with rupture; the volume was so great that the penis could not be seen; yet by a single puncture on each side, in three days the fluid had entirely disappeared, and the parts had regained their orignal size. The simplicity of the operation is such, that I have seen medical men afflicted with hydrocele fearful of having it punctured (through want of confidence in the resources of nature), lest it should prove a failure, without the aid of the trochar and canula.

The principle of puncturing with a fine-pointed needle is not only applicable to promote the absorbtion of the fluid in hydrocele, but in every case of *encysted* dropsy. My worthy and talented friend, Dr. T. Davies, of Broad-street, has lately informed me that he had explored the chest with a grooved needle, in order to convince operating surgeons of the existence of fluid, previous to resorting to the operation of paracentesis thoracis. Having ascertained that fluid existed, he found, to his surprise, that the patients got well by means of the puncture, without having recourse to the usual operation; and so successful has he since been, that he never now employs any other method of emptying the cavity of its fluid. On the same principle the needle may be used with safety and success in many cases of hydrocephalus, and, perhaps, in some tumours.

In hydrocele the accumulation of water proceeds more slowly after puncturation than after the more sudden emptying by the trochar and canula. Acupuncturation, as hitherto practised, both abroad and in this country, has been on the principle of counter-irritation, and as an agent to give discharge to the water in cellular dropsies. The acupuncturation needle is much larger and rougher than the one I use in the treatment of hydrocele by puncturation; in fact, the needle cannot be too fine, provided it be strong enough to penetrate through the integuments, for the smaller the puncture the less pain and inflammation ensue.

From inquiries I have made of the first hospital and other surgeons of the metropolis, I am satisfied that the above plan of the palliative treatment of hydrocele is *entirely new*, and never before practised in Europe, notwithstanding Mr. Kingdon is reported to have said, at the *London Medical Society*, that he had practised it in several cases, some years ago, and at first with success, but afterwards without any effect. This surely must be a mistake, originating in some misapprehension of the words used by him, that gentleman having himself a few months since assured me, when in conversation on the subject, that he had *never* tried it. He is also reported to have said, that a German physician, his preceptor, when performing the operation on my principle, stuck the needle into the body of the testicle, till it broke. The profession is able to judge of the degree of credit due to so unskilful and bungling an operation. I hope that these observations, if made by Mr. Kingdon, will not intimidate the public from availing

themselves of the benefit of my discovery, for, notwithstanding the mysterious story of the bungling German physician, I can with confidence assert that the operation is to be performed by any skilful hand with perfect success, without the least danger of wounding the testicle, especially if the hydrocele be *large*; if *small*, it can be felt, and pushed out of the way. The needle is to be introduced gently, by a rotatory motion, and not thrust in, as some, and, perhaps, Mr. Kingdon, may imagine, like a trochar. I am, Sir, yours truly,

D. Lewis.

28, Artillery-place West, Bunhill-row.
Jan. 9, 1837.

INSULTS OFFERED
TO
SURGEONS IN GENERAL PRACTICE,
BY
DR. JAMES JOHNSON,
In his Medico-Chirurgical Review.

To the Editor of The Lancet.

Sir:—The flag of truce has so long been hoisted between my old friend Jemmy Johnson and yourself, that I fear that you are both forgetting your duty towards each other and to the profession. Flags of truce often beget false confidence, and assist sometimes in covering dastardly assaults upon a foe who could always prove triumphant in the open and fair field of warfare. I do not object to the discontinuance of hostilities between Dr. Johnson and yourself, provided the Doctor is willing four times in a year to assist, by his humble means, your fifty-two annual efforts on behalf of that class to which I belong, namely, general practitioners. In the general politics of the country I do not hesitate to avow that I am a Tory, a thorough-going Tory, and that I would always risk my life and my property in the hands of the enlightened few, instead of allowing all our institutions to be torn to pieces, by yielding up the reins of government into the hands of the impassioned multitude. In the practice of medicine,—in the whole community of practitioners,—we have, not an ignorant but an enlightened body of men. I claim, therefore, for myself and brethren, the right to elect our governors. Accordingly, in *medical* politics, I am a radical, and so is every practitioner who contends for the establishment of one faculty of medicine placed under the efficient control of the representative principle. Since Dr. Johnson failed to produce any impression, except a bad one for himself and his own interests, by his attacks on The Lancet, on the practice of publishing lectures and reports of hospital cases,—by his attacks on the reform speeches of Mr. Lawrence, on the reform efforts of Mr. Wardrop, and, in fact, on the proceedings of all whose efforts savoured of the liberal system of politics,—attacks which, in his articles on the reform principles of Mr. Lawrence, directed no small portion of ridicule and sarcasm against general practitioners,—since, I say, these things were done by Dr. Johnson, which produced so bad an effect upon himself and his interests, and since he induced you to pay respect to the flag of truce which he hoisted, I hoped to observe that if he discontinued his open and direct attacks against the body to which I belong, he would, at least, remain neutral in the field of medical politics, and that he would stick to his trade of book-gutting and analytical reviewing. Several of his late numbers, however, have been so offensive, that I cannot any longer remain a silent observer of his doings. A vein of puppyism—absolutely of arrant puppyism—runs through several papers of the "Review," the insolence and conceit evident in which can, I should hope, hold no relationship with the maturer mind and judgment of Dr. Johnson himself. It is time, however, that the Doctor should look to it, or away will go the remainder of his sale, more rapidly than the first was destroyed by the repeated cutting and smarting operations of The Lancet.

But to the point. I have already said that although I am a Tory to the back-bone in general politics, I am a Radical to the heart's core in medical politics. In a word, I am for a tripartite faculty, acting under the control of the elective or representative principle. If Dr. Johnson speak his true sentiments when he writes in favour of one faculty, which is an out-and-out Radical proposition, and if his own journal, the *Medico-Chirurgical Review*, *honestly* conducted, be a Radical periodical in medical politics, what, I say, does he mean, when speaking of the medical periodical press,—what, I ask, does he mean, by the following sentence, which I quote from an article in his number for January, 1837?

"The *Radical Journal* can command only the services of *low adventurers*, who, *knowing little*, cannot tell us much."

Is this to be received from Dr. Johnson as the description of himself, his writers, his own journal, and his own capability? or is it to be received as a piece of silly, vulgar abuse, which a medical conservative, *professing* to be a medical radical, has directed against the efforts of those among his brethren of the press who sincerely advocate *the interests of general practitioners*, by contending for the establishment of a tripartite faculty of medicine in this country?

It would be useless to enlarge upon the character of the following quotation. *The impression left by it will stick to the memories of thousands of general practitioners*, and will

recal to mind the virulent attacks which were levied *against Mr. Lawrence when that gentleman was struggling in aid of medical reform.* Commenting on the Report of the committee appointed by the Provincial Medical Association, on the operation of the new Poor-Law Act, Dr. Johnson concludes thus:

"We say nothing of the *recommendations* of the committee, partly because that would be unseasonable, and partly because we do not altogether approve of them."

Not *approve* of them! Why *should* a physician approve of them? But there is not a general practitioner in the kingdom who would not run any risk, or incur any responsibility, *if he could but see those recommendations carried into full effect.*

Then, again, regarding the tyranny of the Poor-Law Commissioners, we find the following passage:—

"The Poor-Law authorities are peculiarly circumstanced. A furious and insane clamour has been incessantly directed against the new Poor-Law. A part of the press lives by pandering to the praiseworthy" (*very praiseworthy!*) but "ignorant benevolence of the *mob* of *every* class."

General practitioners, of course, being the most numerous portion, *are the "mob" of the profession.*

In the report of the committee of the Provincial Medical Association, it was stated, very properly and truly, that "the office of *parish surgeon* should combine *the highest qualifications of the medical body;* it being clear that no professional responsibility whatever, public or private, equals it in *variety* and *extent.*" The ground so admirably assumed by the committee is thus *sneeringly* and *contemptuously* opened to the view of the reader by *Doctor James Johnson:*

"The committee, indeed, form a *high* estimate of the qualifications of a *parish surgeon.*"

And on closing the extract descriptive of the qualifications and duties which the *surgeons of the unions* are called upon to possess and discharge, there is *another sneer,* equally contemptuous, equally insulting, equally derogatory, from the high character which surgeons of unions and parishes, being entrusted, as they are, with *the treatment of every description of malady,* are hourly called upon to discharge. Mark, Mr. Editor, the *insolence* of this passage:—

"The combined talents of a Brodie, a Baillie, and a William Hunter, would thus appear to be necessary for the proper performance of the functions of a *parish surgeon.*"

And to the care of "PARISH SURGEONS" is committed, for the present, the body and reputation of DOCTOR JAMES JOHNSON, Editor of the *Medico-Chirurgical Review,* by a general practitioner, once his friend, but who chooses now to adopt the signature of

London, Jan. 4, 1837. CASTIGATOR.

THE LANCET.

London, Saturday, January 14th, 1837.

THE evidence which has now been received from all parts of the kingdom, in proof of the effectual and perfectly satisfactory working of the Medical Witnesses Act, presents the only reward which we hoped to obtain for our labours when we framed the Bill and undertook to conduct the measure through Parliament. It ought not to be forgotten, that the non-payment of medical witnesses at coroners' inquests, and also the numerous unnecessary attendances which medical witnesses were compelled to make at inquests, were two abuses whose existence was not less ancient in date than that of the court itself. Centuries, therefore, passed away while these evils were pressing with extreme severity on the profession; and yet at no period of our history, until the subject was first broached in the columns of THE LANCET, was one word written, that we can discover, in reprehension of those abuses, or in furtherance of an attempt to procure a remedy for them from the legislature.

When, therefore, such evils and annoyances have been allowed to injure the members of the medical profession during so many ages, it may easily be conjectured that a mode of relief at once simple, effective, and satisfactory, was not suggested, until of late, to the mind of any medical reformer who either did or did not occupy a position in which he was enabled to give the force of *law* to his matured opinions on this subject. The pages of THE LANCET were, in truth, the birthplace of the first printed arguments and statements which were proposed in favour of the claims of medical witnesses to remuneration at coroners' inquests. It is, in fact, a question which we have agitated in this journal during many years, and our arguments in favour of a remedy, long had precedence of the general efforts which were

made by the members of the profession in support of their own interests.

When it is seen that the discovery of a remedy was protracted until we had proceeded more than a third of our way through the 19th century, it may readily be imagined that the subject appeared to be one which presented numerous difficulties to the minds both of senators and medical reformers, and that if it were not grappled with at an earlier period, the omission was not so much attributable to neglect, as to a belief that the two evils of *gratuitous* as well as *unnecessary* attendances at inquests, were inseparable from the nature and purposes of the coroner's institution.

Considerations having this character and tendency, necessarily induced us to be cautious in our proceedings. Two great objects were to be attained for the members of our profession,—one, to secure payment for services which were to be performed in a court of justice; the other, to protect medical men from the obligation of making unnecessary, and, consequently, unremunerated, attendances in the court. On investigating the whole subject with the closeness and attention which were absolutely essential to the construction of an adequate measure of relief, the cause of the long continuance of the two abuses which we have described was speedily conjectured, if it were not actually discovered. Numerous were the difficulties which had to be overcome, and the least of them was not found in the fact, that a Bill for the remuneration of *medical* witnesses at coroners' inquests, carried upon the *face* of it, bore in its very *preamble*, a proposition of so *select*, *exclusive*, and *special* a character, that it was to be feared that it would not be sustained by the houses of Parliament, or be tolerated by public opinion.

There certainly is no difficulty in proving that the *greater* amount of good is bestowed on the mass of the community by the new Act; but, in the acquisition of this national advantage, medical men are benefitted to an immense extent, by being relieved, in the first place, from attending unnecessarily, all times and in all places, on the summons of the coroner, without remuneration; and, secondly, they are rewarded for such services as they may actually render at inquests. Thus, advantages, purely of a professional or an individual character, operate so as to produce benefits to the whole community, and the attainment of the latter is directly conducive to the production of the former. These *reciprocal* operations, relative to public and private benefits, are ever at work in the elements of good government.

Having duly weighed the importance of the objects which were to be accomplished, the earliest opportunity was embraced of placing before the profession a copy of the document which it was proposed to convert into a medical emancipation Act. A draft of the Bill was published in this journal, and having been forwarded to the conductors of the other medical periodicals, it was printed in the columns of their works. Thus the Bill obtained the fullest publicity,—was, in fact, submitted to the scrutiny and judgment of the profession during *four* months before it became an Act of the legislature. Had the Bill been smuggled through Parliament,—had it been kept in manuscript until the last moment,—had it, by trick or contrivance, been forced through all its stages in seven or eight days, instead of seven or eight months, *then*, if the Bill had been disfigured by defects, and rendered inoperative by incongruous arrangements, its author, whatever may have been his intentions, would have deserved and received the severest punishment which his injured brethren of the profession could award. But the directly opposite course which he pursued ought, at any rate, to have spared both him and the statute from the misrepresentations of men who had been afforded the fullest opportunity of suggesting any and every amendment in the various clauses of the Bill. The Bill was in Parliament during several months, yet was there not printed in any medical journal, or, in fact, in any other journal, a single sentence which indicated that the Bill was defective. And

why were all the writers silent,—all the hirelings of the corruptionists,—the two knavish fabricators of the PENNY-CLUB JOURNAL,—why, why, we ask, were all these parties silent on the measure, when it would have afforded most of the miscreants inexpressible pleasure if they could have torn the Bill into fragments, and proved its author to be a bungler? Why? This is the answer. The Bill was *not* defective. Its provisions were so suitably and carefully framed, with reference to the objects which it had to accomplish, and with reference, also, to the chances of carrying it through Parliament, that it could not be successfully assailed by a contemptible knot of knavish critics. The Bill, we repeat, was *not* defective. The Act *is not* defective (with the exception, probably, of two of the alterations which were made in the House of Lords), and the excellence, the perfect efficiency, of the measure is proved by the unexceptionable testimony of medical practitioners who exercise their functions in all parts of the kingdom. *Here*, in fact, we discover the real source of the annoyance of which the miscreant conductors of the PENNY-CLUB GAZETTE are the miserable victims. They feel, in truth, that the Act has not only protected and remunerated medical witnesses at coroners' inquests, but that by means of it a brace of hireling knaves have been made to expose their malignant and lying propensities to the whole profession.

The testimony in favour of the Act is unequivocal. It is not possible to elicit evidence of a character more perfectly satisfactory or conclusive. It is really extraordinary that an Act of a character so novel, both in principle and detail, as regards the proceedings of the coroner's court, should have been framed so effectually for the purposes which it was designed to accomplish. But there are three dissentients. Yes; out of nearly FIFTEEN THOUSAND practitioners of medicine in England and Wales, there are THREE persons who have attached their obscure names to objections against the Bill, and *only* three. And what are the complaints of these three? First, an individual styling himself a "surgeon," of Great Yarmouth, states, that having been called to see a patient whom he found had expired, and having seen a bottle in the room which had contained turpentine, he caused an inquest to be held, when, lo! the prime-mover in the inquiry was *not* called as a medical witness, and did *not* get two guineas, for swearing that—*he knew nothing of the patient, the malady, or the cause of her death!* "Therefore," ejaculates wise CHARLES ALDRED, "*these facts* show the *total inefficiency of the Act!*" And they show something besides, which it is unnecessary to mention.

As hero the second in this honourable farce, comes H. SEMPLE, of Islington. And what are his grounds of complaint against the Act? He was the first to demand an order from a coroner for the payment of a fee when he had no legal claim to it, and subsequently he was not called as a witness, after he had cancelled his claim to remuneration by having made a post-mortem examination of the body without the direction of the coroner. "Therefore," exclaims HENRY SEMPLE, "repeal the Act!" but we say, "Repeal HENRY SEMPLE."

Then comes the third dissentient, in the person of the interesting, but not very edifying, Dr. F. H. RAMSBOTHAM. What are the grounds which are adduced against the Act by this objector? He attended two inquests under the Act,—both, as he states, under nearly similar circumstances. In the first case, he got a fee of two guineas; in the second, he got nothing, and, therefore, the Act is "an ill-digested statute."

Here, then, are the names of *all* the objectors, as well as a description of the majority of their objections.

A note will be found at page 572 from Dr. F. H. RAMSBOTHAM, explanatory of a misstatement which appeared in the article on the Mile-End inquest. Mr. PATER, whose polite note we readily insert, has also written to us on the same subject.

After stating that our "assumption" was perfectly gratuitous, and yet that it "is

boldly put forth as a fact," the doctor continues,—"I think it is at least due to Mr. "PATER and myself, if not to the profession, "that you *should publish the name* of your "informant." As we are not in the habit of publishing statements of fact without believing that we possess sufficient authority for so doing, we readily comply with Dr. RAMSBOTHAM'S civil request. Probably the publication of the authority on which our statement was founded, affords to us a species of pleasure with which Dr. F. H. R. is but little acquainted. On that point, however, we shall be able to form a more correct opinion when he has answered a demand which we shall make upon him, precisely similar to the one which he has just made upon us.

1, Inner Temple-lane, Jan. 9, 1837.

DEAR SIR:—I received your note, and a copy of Dr. Ramsbotham's letter to you. I have no objection to its being known that it was I who told you that the *jury* had directed a medical gentleman to be summoned, and that the overseer paid him; for no one can suppose it to be my fabrication, nor, indeed, that any one else *intended* any misrepresentation. My seeing you whilst the matter was in discussion was *accidental;* your mentioning the case to me *accidental;* and *accidental* my having previously heard of it from a friend and client, who had asked my opinion, whether he was right in having advised the overseer, as he had done, to pay only one of the two medical gentlemen; on which occasion he stated what I repeated to you, and which was incorrect, according to Dr. Ramsbotham. Probably it is incorrect, but the Doctor's very animose style indisposes me to give an implicit faith to *his* contradiction, though I must add, that my friend's absence from town has prevented my inquiry into the facts and evidence. Allow me, however, to add, that Dr. Ramsbotham's claim is in no manner affected by *this* question. I am, dear Sir your's truly,

WILLIAM THEOBALD.

To Thomas Wakley, Esq., M.P.,
Bedford-square.

There, Doctor, you have the authority, and we hope that you are satisfied. *Your* demand upon *us* is answered. We now close this article by making a demand upon you, but whether you reply or not, we shall return to the subjects next week which have been introduced by you into a letter that has been printed in the PENNY-CLUB GAZETTE and from which letter the following extracts are taken:—

"*Proofs* are *daily* occurring of the Medical "Witnesses Act being misinterpreted by "coroners and overseers, as well as the "members of the profession."

Come, Doctor, as you are so eager to demand "proof" of the authority on which other persons make their statements, we call upon you, in the face of the profession, and in the presence of that class of students before whom you lecture, and whose morality you are bound to promote and protect, to furnish to us, in order that we may submit them to the profession, the "proofs" which you have the boldness to state are of "*daily* occurrence." From another portion of your letter we take the following extract:—

"Medical men have been summoned by "the coroner, have been kept waiting for "hours in the inquest-room, and have at last "been told by the jury that they are quite "satisfied as to the cause of death, without "medical testimony."

Again, Doctor, we call upon you for your "proofs," your authority for *these* statements also.

What are the names and addresses of the medical men who have been thus summoned?

What are the names and addresses of the coroners?

Whether you shall answer these questions in the next LANCET, or remain sullenly silent, overwhelmed by confusion, we undertake to supply both yourself and the profession with ample proof that *you* have mis-stated the provisions of the new Act,—an Act which, *to your sorrow*, and to that of *your clique*, works most admirably for general practitioners,—in a manner of which any honest and respectable man ought to feel heartily ashamed.

But it must ever be confessed that ignorance and prejudice are sad corrupters of the morals.

When Messrs. Jacob and Harrison, and their confrères, contrive, by their unceasing efforts, to merge the College of Surgeons in Ireland, together with all its capabilities, all its natural and real interests, in the selfish monopoly of their school,—when they impudently assert, that the college and the school are identified by charter; that the fate of the latter necessarily involves the very existence of the former; that every honour and emolument showered upon the school elevates and ennobles the whole profession of surgery, and in a tenfold ratio promotes its interests, even to the newest tyro that has just received his licence;—when the professors do and say all this, they neither act justly nor speak the truth, but are prompted by the basest principles of human nature, viz., self-interest and a love of pelf. When the members and licentiates assent to their bold and mendacious propositions, (and assent they unquestionably do, so long as they suffer them to pass, though in silence—so long as they do not loudly protest against them), they, beyond a doubt, unthinkingly lay the foundation of their own condemnation and destruction, as a body; for if they permit the college to be wilfully confounded with, nay, merged, as at present, in the more prominent school, is not that risking the *being* of the college upon the *merits* of the school? Do they not plead the same issue, do they not court the same judgment, and if united in the same judgment, shall they not fall in the same *condemnation*? For what measure of reform, emanating no matter from what government, can spare a *novel* institution so worthless, so injurious, so unjust, so impolitic, so corrupt, as the private theatre in York-street, farcically called the College School?

It is *worthless*, because it promotes no single earthly object (always excepting the *sacra fames auri* of the professors) that is not equally well, if not better, attained by any *other* private school. It is *injurious*, inasmuch as it occupies the place, the attention, the time, the revenue, the exertions of the college, all of which are thereby necessarily withdrawn from objects of utility; it is injurious, also, as being the fruitful source of much jealousy, division, and enmity, in a body which ought to be unanimously devoted, in all brotherly love, to promote the interests of science and the profession, the guardian of both of which it is presumed to be. It is *unjust*, because it confers upon a small number of men who are distinguished by no superior merit, but only by either skill in intrigue, or a cringing submission to those who possess that unenviable talent, not only a pre-eminent position, but also pecuniary, and other valuable considerations, which, in the race of teaching and of practice, give them most unfair advantages above their fellows, who, to say the least, are equally meritorious.

Like every other monopoly, the college school is *impolitic*. To prove it impolitic, it is barely requisite to state the fact, that it is a monopoly. Every tyro in political economy knows that this country, and this age, have long passed the period when protective duties (and there is a complete analogy on this head) can be productive of any thing but injury to the community. It were an easy matter to prove that education and science would long since have assumed, in Ireland, a much higher tone than at present, were it not for the impolicy of having a school attached to the college. If the college would abandon its school, as it ought to do,—if it would leave the trade of teaching *perfectly free*, and confine its interference with elementary education, to its only necessary and proper duty, that of appointing a fit court of censors, who should be above the insinuation of corrupt motives, and capable of testing the acquisition of knowledge in candidates, *and not the professors themselves, or their incompetent creatures*,—then, and not till then, open competition, and the demand for knowledge instead of the peculiarities of empty dogmatism, would soon summon to every chair in every private school, men of genius, talent, and attainment, who would form a striking contrast, indeed, to the unfledged and unlettered

youths who daily spring up into professors travestie, under the baneful influence of the present vicious system. Then the college would be an instrument of usefulness; it would be freed from a paralysing incumbrance, and it would be capable of turning its energies and resources to objects of utility. Let the college apply such a stimulus to education; let it require *knowledge* for its licence; let it excite a philosophic taste. It is quite in the power, and it is the proper province, of education, duly applied, to infuse into the minds of youth a taste for philosophy; let it create a demand for knowledge, and afford a remunerating market for the commodity, and then it will apply a truly gigantic power to the promotion of science, not in Dublin merely, but in every part of the kingdom to which its licentiates and members migrate.

When an institution, in itself useless, injurious, unjust, and impolitic, is, nevertheless, upheld, and upheld against the judgment, the wishes, and the interests of any community, it is unnecessary to say that such institution must be *corrupt*, and maintained by *corrupt* means: That the college school is not only corrupt, but the cause of corruption in the college, is a notorious fact: it is unnecessary to waste time or space in proving a self-evident proposition; suffice it to say that it is corrupt, and must reap the penalties of corruption, to be unflinchingly inflicted in these reforming times,—*it must die the death*. It is no difficult matter, then, to divine the schoolmen's scheme in identifying and wilfully confounding *their* school with the College of Surgeons. So long as this illusion, absurd and unjust though it be, lasts, they will retain their unmerited pre-eminence,—their cupidity will be gratified. That satisfies them for the present. By-and-by, when the day of retribution comes, as come it must, they hope, by continuing their scheme, that the college will possess sufficient buoyancy to save them and their job from sinking in the torrent of reform; they care not, however, if they involve the whole corporation in their own deserved ruin. Such is the scheme of the schoolmen. But the members and licentiates! How shall we designate their folly and infatuation? Will they still continue their apathy? Are they content to run the risk to which we have alluded? Will they rest satisfied with the result? We can only marvel at their *suicidal* inaction.

INTERCEPTED LETTERS.

ADVICE TO A YOUNG LONDON SURGEON.

Letter I.

"Dear Sir:—As you have been referred to me by my esteemed friend Sir Henry, for me to tell you how you ought to proceed to practice as a *pure surgeon* in London, I do not think it would be generous in me to refuse your request. It will cost me a good deal of trouble to put down all that I have thought, felt, and seen, on this important subject; and with all my experience, I am quite aware how incompetent I am to execute the task, when my qualifications are compared with the acknowleged talents, integrity, and high moral feeling which Sir Henry has shown in his 'Advice to a Young London Physician.'

"To prevent any misunderstanding or perversion of my sentiments on professional matters, it is, perhaps, better that I should put them down in writing. Although I consider my correspondence with you to be quite confidential, yet I am well aware how strangely these things meet the public eye sometimes. I confidently hope, however, that you will observe the greatest circumspection in the use which *you* may make of my communications, either during my life-time or after my decease; not that I am ashamed of any of the sentiments or maxims which I shall endeavour to impress on your mind; but because in performing with integrity the duty which I have imposed on myself on this occasion, my conscience imperatively calls upon me to repeat conversations and narrate anecdotes of living individuals, who may feel offended at my mentioning their names. But should you, for any purposes of your own, be induced to repeat my maxims, be sure not to mention them on my authority.

"You must also be well aware that the points to which I must call your attention are very numerous, and cannot, therefore, be fully discussed except in a series of letters. My time, like that of all professional men, is not my own; I must, therefore, write to you on the various subjects which are to be discussed, as often as I can find leisure. You must not expect to find order or method in my epistles, but be contented with my arranging topics as they become suggested to my mind.

"I do not think that I could offer to you a better example of *what ought to be* the whole course and progress of a PURE LONDON SURGEON than my own, for I have been very successful, and I like to think and tell of my own progress in life. It is a laudable vanity to receive pleasure in the remembrance of those days when I had nothing in the wide world to look forward to but my own exertions, and compare the events of that period with what happened to me yesterday, when I was at Court, kissing the hand of a beloved Sovereign, on bended knee, in Court attire, with a full bag-wig, Brussels lace frills, a white satin waistcoat richly embroidered, diamond buckles, a *chapeau bras*, and a broad blue ribbon suspending the insignium of the high order of knighthood to which I have the honour to belong. Indeed, I cannot help contrasting my present high position and my 'improved appearance' (her ladyship's words) as a courtier, with my *debût* some thirty years ago as a student at St. George's Hospital, immediately on my arrival from Salisbury, before I had either time or money to be equipped by a London tailor. Perhaps I was just as happy and contented in those days, with my leathern gaiters and rural cords, as I was yesterday, when surrounded by the pageantry of the Court of WILLIAM THE FOURTH! I will not conceal from you the difficulties I have had to overcome in performing the perilous journey of life, nor will I refrain from telling you how I have been able to surmount them, and describe the various means by which I have attained my present distinguished station. Few, I am well aware, have the courage to confess, even to themselves, their errors and frailties, and the various arts which we must all have recourse to in order that we may secure a full share of worldly success. But I have nothing to fear from disclosure, and there is no action of my busied life of which I repent. Though I have always had an eye to fortune, I never got too fond of the world. I soon found out what both men and women were composed of, and I determined to make the most of them. Depend upon it there is great difficulty in passing along the stream of life honestly. I remember Sir W. KNIGHTON once saying, 'honesty,' my dear friend, 'is the most *curious* commodity in which you will have to deal;' a truth, the force of which I have ever since strongly felt. No doubt we must all sometimes yield to circumstances.

"The present letter I design to be merely introductory; it is, therefore, brief. Having thus commenced my task, I shall take the earliest opportunity of explaining, at sufficient length, the course you should pursue, and the means you should adopt, in order that you may become a distinguished Pure.

"B. B.

"Saville-row, January, 1837."

WESTMINSTER MEDICAL SOCIETY.

Saturday, Jan. 7th, 1837.

NEW INSTRUMENT FOR PREVENTING AND RELIEVING PULMONARY AFFECTIONS.

The Society resumed its meetings, after the vacation, this evening. The attendance was rather small.

Dr. JOHNSON introduced to the notice of the Society a very ingenious apparatus, invented by Mr. JULIUS JEFFREYS, a surgeon, lately in the East India Company's service. The object of the apparatus, which is denominated "The Respirator," is to ensure to the wearer a supply of atmospheric air, of a certain temperature, under all vicissitudes, and to obviate the danger of sudden changes of either. Persons labouring under phthisis, asthma, and other affections of the lungs, feel alterations of temperature very acutely, more particularly changes from heat to cold, and it had struck the inventor of the apparatus that if a means could be devised for keeping up a supply of warm air to the lungs, a vast deal of discomfort and disease might be prevented. The lungs are continually giving out the warmth which might save them from injury, but to no purpose, for it is carried out with the breath, and lost in the atmosphere around. The air which issues at each expiration contains more than enough heat to warm the following draught of inspired air. To transfer the warmth from the former to the latter became the grand desideratum, for which object some instrument was required. To achieve its purposes, this instrument must be worn on the face, to catch with rapidity all, or nearly all, the heat of the breath, to store it up, and to impart it to the fresh air drawn in, and make the mouth or nostrils comfortably warm. The Respirator consists of a packet of lattices, or small frames of silver, kept apart by other similar lattices, of a waterproof substance. These last lattices serve as imperfect conductors of heat, for keeping the silver lattices apart. The silver lattices are overlaid with gold wires on both sides. The wires on those nearest to the mouth are one three-hundredth of an inch thick, and one three-hundredth of an inch apart. On the outer lattices they are about one six-hundredth of an inch thick, and one six-hundredth of an inch apart. The packet is made up of from four to ten of these lattices, carrying from eight to twenty layers of wire, with a non-conducting lattice between each skein. The packet is arched, to suit the curvature of the mouth. It is lodged in a case, which it exactly fits everywhere but behind, next the mouth. Here there is a small space behind the packet, to permit the breath to diffuse itself to every part of the back, or concave surface, of the packet,

after it has passed from the mouth. The case is so formed as to collect whatever moisture is condensed from the breath, and to deposit it on a strip of sponge, fixed in the lower part of the case. It is only necessary to open the case, and remove the packet of skeins once a day, in order that the interior of the case and the sponge may be removed, and rinsed in warm water. There is another kind of instrument, which has a communication with the nostrils, as well as the mouth, to suit the convenience of persons who have not acquired the habit of breathing chiefly through the mouth.

Such is a concise account of the "Respirator." A conversation, rather than a debate, took place on the subject. Several objections were started, chiefly by Dr. Williams, and it appears to us that the clearest way of putting the subject is by stating all the objections *seriatim*, and appending the several answers of Mr. Jeffreys immediately to each objection.

First objection.—The Respirator must present some obstruction to the passage of air through it, and it was thought by one or two members that they could perceive, to a certain extent, that it impeded the breath. The invalid would, therefore, derive little benefit from the oral respiration, which, by such obstruction to the breath, would induce him to breathe through the nostrils.

Answer.—The objection is rather founded on the appearance of the instrument, and upon a nervous feeling on first applying it, than on any real obstruction, since a large majority of the persons, both invalids and others, who have tried it, have declared that they could scarcely perceive any difference in the freedom of breathing when wearing, and when not wearing, the instrument. It was a fact, that although each passage between the wires was only one three-hundredth of an inch, the whole, collectively, amounted to a free space of nearly two square inches, or about twenty times the area of the narrowest portion of the larynx, through which the breath must pass. The brightness of the metal, and the fineness of the workmanship, tended to dazzle the sight, and to make it appear as if the whole space were occupied by metal, whereas it only occupied the half of the space. There was no desire to maintain that no obstruction existed, but the question for consideration was, whether the obstruction was not infinitely less than that which is offered to the passage of cold air down an irritated larynx. Experience had decided that invalids, who could not draw in a few cubic inches of cold air without dyspnœa and cough, were able in the coldest air to draw full and deep inspirations through the Respirator, with perfect ease. The trifling obstruction in the instrument, if, indeed, it were perceptible, was nothing in comparison with the resistance offered by an irritable larynx to the passage of cold air.

Second objection.—If the instrument, as stated, raised the respired air to the full summer temperature, such warmth would tend to induce congestion in the bronchial surfaces.

Answer.—A warm atmosphere alone could not have such a tendency, since affections of the bronchial surfaces were by no means common in the hottest climates, even where the transition from night to day amounted often to forty degrees, as in the valleys of the Himalayan mountains. The question was not, however, whether warm or cold air was the suitable medium for the patient to be surrounded by, but whether or not, that medium should be of an equable temperature. No physiologist would recommend the pulmonary invalid to expose himself to sudden and great alterations of temperature, although people in health might bear them with impunity. The state of the case was this,—the habits of the people of England placed them, from their youth upwards, in houses which were artificially warmed, and, commonly, in crowded apartments, where the temperature, in winter, was oftentimes at the summer heat. Since, then, the invalid will not, however proper it may be, live in an open shed, which would afford him all the benefits of cold air, he must suffer the trial, admitted on all hands to be injurious, of a sudden and great change of temperature, when going from a warm room to the open air, and when returning from the cold air to the warm room, unless he will wear a Respirator, which performs the simple duty of supplying him with air of the same temperature without, as that which is within doors.

Third objection.—The invalid within doors is wholly surrounded by a warm air, acting equally on the surface and the lungs, but with the Respirator on, out of doors, the lungs would be supplied with warm air, while the surface of the body would be exposed to the cold of the climate. This would be prejudicial.

Answer.—The body is never exposed to the climate, for every invalid is well clad in woollen, which keeps the surface of the body in an artificial climate, the thin stratum of air between the skin and the clothes being everywhere above 80° of heat; and it is highly desirable that the air entering the lungs of the invalid should be as warm as that enveloping the surface of the body.

Certain alterations were proposed by members in the structure of the instrument, such as the use of pierced plates of metal, and the assemblage of the metal in the manner practised in the oxy-hydrogen blowpipe. Mr. Jeffreys replied to these, that he had given the subject so long a consideration, and had instituted so many experiments on the subject, as to enable him to

affirm with confidence that such alterations would be found as defective in practice as they certainly were in principle, and he showed to the Society a "half-action" packet, which was the best form of construction, where less power was desired. In this packet the same alternation of conducting and non-conducting laminæ had been adhered to, it being a part of the principle from which he would on no account deviate, but the number of the laminæ is smaller than in the "full-action" packet.

ABSORPTION OF POISON BY PUNCTURED WOUNDS.

Mr. G. Bird related the following case:—Last August he was suddenly called to see a young man, twenty years of age, a butcher, whose family were subject to pulmonary affections. The patient had been well previously. He found him with an anxious countenance, and pale, and suffering great pain on inspiration, accompanied with the "catch," which is observed in pleurisy; dyspnœa, great heat of the skin, and cough; pulse small, weak, and jerking. On examining the chest, no ægophony was found, nor any symptom of pleuritic affection. The pain was confined to the upper part of the left side of the chest; no pain on the right. In the left there was a slight crepitating râle, not altogether that which is distinctive of pneumonia, but one which was rather *felt* than *heard*, probably arising from effusion into the cellular tissue, but it nevertheless threw him (Mr. B.) off his guard; and from some of the family having died of disease of the lungs, he requested another opinion. Accordingly Dr. Addison was consulted, and it immediately struck him, that the symptoms were the result of a poisoned puncture. On the ring-finger of the left hand a small puncture was detected, containing pus, with a red mark around it; there were lines extending up the arm, with tenderness in the axilla. It was then concluded to be a case of inflammation of the absorbents. The prognosis was unfavourable. Leeches were applied on the pectoral muscle, on the left side; the bowels were opened, calomel and opium were given, and poultices were applied to the finger. The lad improved, and soon got quite well. He remembered having punctured his finger in the slaughter-house, a day or two before. Another case had occurred to him (Mr. B.), in which he found the patient labouring under similar symptoms, from a similar cause. In this case the axillary glands were enlarged, there was a small discharge from the finger, and a red line up the arm. In the first case the pus had been confined by adhesive plaster. The same plan of treatment was adopted as in the first case, and the man recovered. This puncture was also received in a slaughter-house. In this instance the pus had a free exit. He (Mr. B.) did not know that these cases were common among butchers, although wounds received in the dissecting-room were well known to produce severe constitutional affections.

Mr. Shuter thought he had observed that constitutional symptoms were the result of confined secretions in punctured wounds. He had found that giving a free exit at once to those secretions, produced relief. It was remarkable that some subjects brought into the dissecting-room affected many persons, whilst many other subjects never seemed to exert any influence through punctures.

Mr. Pettigrew, jun., mentioned a case in which severe constitutional irritation arose from the absorbtion of gonorrhœal matter by a wound in the hand.

London Medical Society, *Jan. 9th*, 1837.—At the annual time of meeting (the first after the recess), so few members were present, supposed to be owing to the numerous cases of illness prevailing, that the Society adjourned until Monday evening, Jan. 16.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, January 10, 1837.

Mr. Earle, President.

A paper by Dr. Hume Weatherhead was read, the object of which was to illustrate and determine

THE ESSENTIAL NATURE OF INFLAMMATION,

by an analysis of its phenomena, whether symptoms or results, deducing them as one consecutive series of effects, dependent for their relations on one another.

The author prefaced his remarks by observing, that all the phenomena of the living system, both morbid and healthy, owed their origin to nervous influence—that it was the nerves that determined, in the first instance, the rush of blood to a part inflamed—that the increase of temperature as a symptom of inflammation was the result of this afflux—that from the same source proceeded, directly and indirectly, the tension and tumefaction, and that the attendant pain had a similar origin. Blushing, he said, presented the penumbra of some of the primary symptoms of inflammation. Nervous excitation (for a moral emotion is of this nature), throws the blood on the cheek, and the part burns with a sensible heat. In blushing, the increase of vascularity fades as the emotion giving rise to it subsides; but the phenomenon was instructive, inasmuch as it proved how readily vessels can, to a certain extent, dilate, and afterwards contract to the natural size, without any morbid consequence ensuing. The eye, when inflamed, presents the symptom of

vascular turgescence very manifestly, shewing the red globules getting into vessels that did not carry such in their natural state—and here the author pointed out the reservation with which the term "red globules" required to be accepted, by showing that this optical property depended, in a great measure, on the size of the globules themselves, and cited the vessels of the capsule of the lens as beautifully illustrating this fact.

The *first* effect of an excitant or irritant, he went on to observe, was to attract the blood to the seat of the irritation; but if this effect were kept up beyond a certain period, or carried beyond a certain degree, while the excitation continued to attract as much blood as before, the power of the capillaries to forward it diminished proportionately to the exhaustion produced by their prolonged over-action; as long as the increased action in the capillaries was in a ratio with the increased supply of blood, inflammation could not be said to exist; but as soon as the inverse of this took place, by which one became disproportionate to the other, that very circumstance, of itself, constituted and established that morbid state which we call inflammation.

The author next defined the distinction between this state and congestion. In the former the congestion existed in the capillary arteries, in the latter the turgescence had its seat more particularly in the capillary veins.

The author proceeded to treat of and exemplify the power of the arteries to determine blood to particular parts, independent of the heart, and then deduced the increased temperature of a part inflamed, from the difference of capacity for caloric which the blood underwent in changing from arterial to venous, and how the release and evolution of the latent caloric, which went on in the capillary system, kept up the temperature of the surface in health, and how its inordinate evolution alone made it a morbid symptom, as is most conspicuously as well as conclusively exemplified in scarlatina. However, while there existed in the acute stage of inflammation an increased determination of blood to a part the moment it entered vessels not intended, in their natural state, to convey red globules, a state of hæmostosis necessarily ensued. From this state of hæmostosis proceeded an effusion of serosity into the cells of the contiguous cellular membrane in common phlegmon—or into serous cavities when serous membranes were the seat of inflammation. It was from this circumstance (most manifestly exemplified in certain vesicles by the effects of a blister), that the author adduced proofs that the pus was nothing else than serosity of a particular quality, which, by a spontaneous change that its albumen undergoes, and a partial absorption of its lymph, was converted into pus—an opinion which he sustained by quoting the celebrated experiment of Van Swieten on ulcers,—the purulent change which serosity undergoes under our eyes in various exanthematous pustules,—how promoters of serous secretion were also promoters of suppuration,—and, lastly, how strictly pus and serosity were identical in their chemical composition.

The author next took a review of anasarca and its causes, as furnishing additional confirmatory illustrations of the morbid physiology of the capillary system—how both œdema and anasarca invariably proceeded from vascular hæmostosis, and that the former could be produced artificially, as by a ligature—why the fluid effused in these diseases did not change into pus (the deficiency of that which forms the globules of purulent matter—albumen).

He next explained the pathological formation of an abscess, elucidating his views on the subject by analogy with the change of a vesicle into a bleb—the breaking down of the cellular septa—how that tumefaction was always greatest when cellular membrane most abounded and was most lax—how serous and fibrous tissues seldom suppurated, (and, when they did, it was always sparingly,) and why a common phlegmon should readily suppurate, and a boil should not.

Pain, as a consecutive symptom of inflammation, next came under notice, and it was shown this was mainly attributable to the distended state of the blood vessels extending or stretching the tissue or structure in which the inflammation was seated, and that, *ceteris paribus*, the pain was always most severe in those tissues which yielded with the greatest difficulty.

Gangrene and sphacelation were next briefly referred to, as results, rather than phenomena, of inflammation; since both are to be regarded, the one as a partial, the other as a total, extinction of the vital powers of a part,—results that may be induced without any previous inflammation. The paper closed with some detached observations introduced as corollaries, and referring chiefly to serous effusions. The author was inclined to regard the principal function of the cells of the lax cellular membrane, as reservoirs spread over the surface of the body to receive a portion of the circulation, when, from some morbid cause, the blood in the superficial capillaries was stopped or impeded in its course. The splanchnic cavities performed a similar office to the large viscera when obstructed. The author concluded his observations by narrating an unusual case of ascites from scirrhous liver, in which several of the exhalants opening on its anterior aspect, were so enlarged as readily to admit the end of a probe.

It was proposed and carried, that the discussion on the above paper should take place at the next meeting of the Society.

UNIVERSITY COLLEGE MEDICAL SOCIETY.

Friday, January 6th, 1837.

Mr. MORTON, President.

INFLUENCE OF OCCUPATIONS ON LONGEVITY.

Mr. DARVILL commenced a paper on this subject, with some remarks on the generally admitted influence of occupations on health, extolling those which are followed in the open air, and require muscular exercise. The inhalation of powder, or dust, of any kind, excited pulmonary disease, and injurious effects were ascribed to various vapours and changes of temperature. The author then drew attention to the supposition that literary or mental pursuits, were unfavourable to health and longevity. This, he thought, was erroneous. Blumenbach used to assert that he never knew any one die from hard study. The great labour and age of many German writers, confirmed the assertion. Dr. Madden's work on the "Infirmities of Genius" was then referred to, and Mr. Darvill said he considered that the Doctor's mode of arriving at his conclusions was not correct; the tables on which he founded those conclusions, were drawn up from observations made on only twenty individuals in each class. Thus he had given a list of twenty medical authors, to prove them longer lived than twenty imaginative writers; he had omitted to note, in the former class, some celebrated men who had died at an early age, as Bichat, Beclard, Georget, and Godman, neither of whom reached the age of 41, whilst amongst the latter the names of Gœthe, Chaucer, Wieland, and Klopstock, who all died very old, were omitted. Mr. D. therefore disputed the conclusions of Dr. Madden, and thought that it might be established as a general axiom, that literary pursuits were directly favourable to long life, whether requiring exercise of the memory, the judgment, or the imagination, and that when the health was apparently injured, the evil was dependent rather on collateral circumstances, as irregularities in eating, drinking, sleeping, &c., acting upon unusually susceptible frames. He substantiated this view by referring to the lives of poets and imaginative writers who had lived for but a short time, tracing their early deaths to their injudicious conduct. The author concluded with some remarks on the due regulation of the functions of the body, in order to ensure the continuance of health during literary exertions, particularly adverting to the advantage of adopting unexciting diet, using a certain degree of muscular exercise, and taking a full quantum of sleep.

Mr. GEORGE thought that studious habits were injurious to health; hundreds of students probably perished before they could attain a name. It was only from known writers that any inferences could be drawn with respect to the influence of their pursuits on their longevity.

Mr. HOBSON made some remarks on the habits of literary men, which he thought were too often over indulgent, and creative of disease. Excitement of the brain, without exercise of other organs, must be injurious.

Mr. TOYNBEE observed, that the existence of a high degree of excitement in the brain during mental exertion, was confirmed by the fact, that precocious children possessed skulls which were remarkably ossified.

After a few observations from other members the meeting was adjourned.

LETTERS RESPECTING THE DECISION UNDER THE

MEDICAL WITNESSES ACT

AT THE

LAMBETH-STREET POLICE-OFFICE.

To the Editor of THE LANCET.

SIR:—Having been a subscriber to your valuable Journal for many years, I trust you will not consider me unnecessarily troublesome in begging you to correct a statement which certainly, in some of its points, is not strictly true. The article to which I allude is contained in THE LANCET of last week, and headed "Lambeth-street." Your remarks, I presume, would lead us to conceive that, in addition to Dr. Ramsbotham and myself, a *third* person was employed. You state "the jury selected a witness whom the coroner summoned, in addition to Mr. Pater, and that witness was paid his fee of two guineas." This was not the case. The jury summoned no one; but Mr. Baker, at my request, gave his order both to the doctor and myself, before a jury was empannelled, and the reason for my requiring the services of Dr. Ramsbotham is best explained by the following statement:—

Some weeks ago I was called upon to attend a servant of Captain Burton. The mistress had a suspicion that the girl was pregnant, and had spoken to her seriously on the subject. She, however, from time to time, denied the fact, until the morning on which I saw her. The preceding night Mrs. Burton became more convinced her suspicions were well-founded, from her pacing the room under which she slept, apparently in great pain. She went to her and again accused her of her state, which she again denied. In the morning, her mistress finding her size reduced, was persuaded something had taken place, and immediately sent for me. Mrs. Burton having related the obstinate denial of the girl, and naming her reasons for thinking she had been delivered

in the night, requested me to pass my opinion. I went to her chamber, and on examining her abdomen found the uterus contracted, with every other appearance of its having expelled its contents within a few hours. I told her it was of no avail to deny the fact—it was apparent. To my interrogations respecting the infant, she gave evasive answers, totally disclaiming all knowledge of what had taken place. On this I returned to Mrs. Burton, and assured her there was not a doubt of the circumstance having occurred. Search was then made, and in a slop-pail, concealed with dirty clothes, was discovered a full-grown child, apparently expelled head downwards into a quantity of water, with a lacerated umbilicus. Fearing the girl had been guilty of infanticide, and not wishing to be the only witness in her condemnation, I recommended to Captain Burton (who wished to act with lenity towards her) the propriety of consulting Dr. Ramsbotham, who is a lecturer on medical jurisprudence at the London Hospital. He was accordingly called in, and representing the serious nature of my impressions to Mr. Baker, and the probable result of the investigation, he at my request granted *two orders* for a post-mortem examination, one for the doctor and another for myself. This additional order to Dr. Ramsbotham, it appears, cannot be issued without the sanction of a jury, and, consequently, the overseers refused to pay it.

This, Sir, be assured, is a correct detail of the affair, and you will oblige me by making such alterations in your former statement as your own liberal judgment may suggest.

I am, Sir, your obedient servant,

W. G. Pater.

6, Hardwick-place, Commercial-road.

To the Editor of The Lancet.

Sir:—In your strictures on a report of some proceedings at Lambeth-street Police-office, last week, which I have seen this evening for the first time, there are *two misstatements*, and I shall feel obliged by your correcting them in the next number of your journal.

One of these you were led into by the inaccuracy of the report itself. It seems to have been stated in the papers (I quote from The Lancet), that "the coroner directed Mr. Pater to examine the infant, and give evidence as to the cause of death; but Mr. Pater, not wishing to depend on his own judgment, called in Dr. Ramsbotham, when they both examined the body, and afterwards were summoned by the coroner to give evidence." The facts are just the reverse. The coroner, as soon as he was apprised of the circumstance, issued *two distinct* orders at the same time,—one to Mr. Pater, the other to me, (the latter on Mr. Pater's request, as I had seen the child with him soon after it was discovered), directing us to make a post-mortem examination of the body; and we acted according to the instructions we received from him. For your satisfaction, I enclose Mr. Baker's "order" to me to make the anatomical inspection, and to give evidence, as well as his "order" on the parish-officers, to pay me two guineas. A duplicate of this paper was sent to Mr. Pater.

The other misrepresentation *you alone* are responsible for. You say "that the report is otherwise incorrect," in reference to a supposed misapprehension of what fell from Mr. Hardwicke, "may be inferred from the mention of a fact, which, if it had been stated, would have given a totally different character to the whole transaction. The circumstance to which we allude is this:—Under the powers which are conferred by the second clause of the Act, *the jury* selected a witness, whom the coroner summoned, *in addition* to Mr. Pater, *and that witness was paid his fee of two guineas.* Mr. Pater was offered his two guineas. We think that the profession will agree that this was 'liberal' and proper conduct on the part of the parish. But, then, up starts Dr. F. H. Ramsbotham, and *he* claims a third fee of two guineas. Yet, be it observed, Dr. F. H. Ramsbotham attended at the post-mortem examination, *not* by the request of the jury, *not* by the direction or summons of the coroner, but at *the request* of Mr. Pater—in fact, as the 'friend' of Mr. Pater."

The assertion contained in the first part of this paragraph is "totally" *void of foundation*; and nothing transpired either in the jury-room, the police-office, or elsewhere, which could possibly lead to such a *glaring mistake. No medical man was selected or summoned by the jury*, none of the jury hinted at the necessity or propriety of such additional testimony,—no person was present when the dissection was being made, except Mr. Pater, myself, and Mr. Pater's apprentice,—who took down notes from our mouths, while we prosecuted the examination, but who was not present at the inquest. No other medical man but ourselves either gave or tendered evidence, nor was any other member of the medical profession at all mixed up with us in the affair; so that your assumption is perfectly gratuitous, and yet it is boldly put forth as a "fact," which had been kept in the back ground. I think it is at least due to Mr. Pater and myself, if not to the profession, that you *should publish the name* of your informant.

I have already answered the allegation in the second part of the sentence, by showing that I did not undertake the examination *at the request of Mr. Pater;* but that I was associated with Mr. Pater for that purpose *at the express command of the coroner;* and a

fine of *five pounds* hung over the head of either of us who disobeyed the summons.

I am, Sir, your obedient servant,

FRANCIS H. RAMSBOTHAM.

14, New Broad-street,
Jan. 4, 1837.

P. S. In reading over your remarks again I find an insinuation, which might convey the idea that I was a personal friend of Mr. Baker, the coroner. I cannot boast of that honour, as I never exchanged a word, or any other kind of communication, with Mr. Baker, until the inquest in question; nor did I even recollect his person, when he entered the room on that occasion, although I had seen him some years ago, during his canvas for the coronership.

TREATMENT OF STUDENTS AT AN OPERATION IN DUBLIN.

To the Editor of THE LANCET.

SIR:—I request that you will be kind enough to give insertion, in THE LANCET, to the following account of a most outrageous assault which was lately made upon the pupils at an operation in the *Jervis-street Hospital* of this city. At the time appointed a number of students assembled at the hospital, and proceeded up stairs to the operation room, but were prevented from entering it by a man who was placed on the summit of the stairs with a bucket of *whitewash* and a *brush*, with which he said he had "orders to bespatter any gentleman who attempted to pass on to the lobby." An immense crowd soon mustered on the stairs, and considerable uneasiness was manifested at the delay and this treatment. Several attempts were made to pass into the room, and as many times were the gentlemen actually *bedaubed with this whitewash*. Mr. Ellis entering at this time (loudly cheered by the students, who had made way for him) was near meeting with the same fate. Eventually the man was dislodged from his hold, and several made their way into the room. But his place was taken up by—no other than *one of the surgeons of the hospital*, a Mr. Stapleton, who with the most ruffianly demeanour defied any one to pass *him*. Now, whatever was the object of delaying the admission of the pupils into the operation room, it could not possibly justify the adoption of such means of attaining that object. Who ordered the man to this occupation we know not; but that Mr. O'Rielly and Mr. Stapleton sanctioned it, is proved by the fact of their being witnesses of it. Not being present at the operation myself, I waited in the hope of seeing the affair noticed, and I must say I am sorry to see so little spirit of resistance to injustice exhibited among so large a number of medical students. However, as the insult was offered to the pupils as a body, I consider that as one of that body I am justified in resenting it. These facts may appear almost incredible, but numbers can testify to their accuracy.

I remain, Sir, your obedient servant,

J. R. COOPER,
One of the Pupils of the Hospital.

Fannin's Library, Grafton-street, Dublin,
Jan. 2nd, 1837.

OBSERVATIONS ON MR. EVERITT'S REMARKS ON

THE NEW PHARMACOPŒIA.

By RICHARD PHILLIPS, *Esq.*, F.R.S.L. *and* E., &c., *and Lecturer on Chemistry at St. Thomas's Hospital.*

To the Editor of THE LANCET.

SIR:—Permit me to make a few observations on Mr. Everitt's criticism on the *London Pharmacopœia*, intending to prove the folly of directing medicines to be prepared on a small scale, because they can be obtained more cheaply on a large one.

Mr. Everitt says, "Thus, A is a maker of carbonate of soda from salt; he gets hydrochloric acid at the same time. B is a maker of sulphate of magnesia from bitters (bittern); he also gets hydrochloric acid at the same time; and this acid, procured from distilling sulphuric acid with muriate of magnesia, is just as good as that obtained from salt by sulphuric acid, provided care be taken to have it pure."

Now, it is readily admitted, that provided two portions of hydrochloric acid are equally strong and pure, they are equally good, but it does not follow that the modes of procuring them are equally eligible; and until bittern can be obtained free from common salt, the sulphate of magnesia procured from it by sulphuric acid, must be mixed with sulphate of soda. This case has, I grant, but little to do with the processes of the Pharmacopœia, but it proves that Mr. Everitt is not happy in the selection of his illustrations.

Mr. Everitt afterwards says, "We shall now give a few instances illustrative of the *great care with which the work has been executed;*" (the italics are Mr. Everitt's, and were not required to show that he meant the great *want* of care); he then mentions "*ammoniæ liquor*, refined, (defined) 0.960," and proceeds to state, that only two fluid ounces of water should be added to one fluid drachm (meaning fluid ounce) of the *liquor ammoniæ fortior*, to reduce its sp. gr. to 0.960.

When Mr. Everitt's correction is corrected, I admit that he is right; and it was originally stated that these proportions should be employed; the error is, I believe, a misprint of *tribus* for *duabus*. It is one

which I presume Mr. Everitt will pardon, for by an error, somewhat more difficult of commission, he states that "much of the commercial *liq. ammoniæ*, contains chloride of *colchicum*, meaning *calcium*."*

With respect to the present method of preparing liquor ammoniæ, Mr. Everitt says it was "perhaps the worst of those in the old edition;" and he adds, that "the alterations are merely to suit the new measures;" that is, the alterations are no alterations at all. This is, however, a very incorrect representation of the circumstances of the cases. In the late Pharmacopœia, 8 ounces of hydrochlorate of ammonia, 6 ounces of lime, and 64 fluid ounces of water, were directed to be used to procure 12 fluid ounces of liquor ammoniæ. To make a liberal allowance, let it be supposed that one-fourth of the water remained diffused through the lime undissolved; we should then have to heat 48 fluid ounces of solution to procure 12 fluid ounces of liquor ammoniæ; whereas by the present process the same quantity of solution yields 18 fluid ounces of product. In this operation, therefore, smaller vessels, less fire, and time, are employed than in the former one.

Mr. Everitt then shows, and I admit the accuracy of his statement, without allowing that of the inference deduced from it, that in the present process half the ammonia of the muriate is lost; and he proposes a method by which this is to be remedied. Although an experienced person might advantageously employ some modification of this plan, especially if metallic vessels were used, yet I prefer the loss of the ammonia to the complication of Mr. Everitt's apparatus, and the danger which is incurred in distilling under pressure with glass vessels. And I assert that it was not from want of care that a process similar to that proposed by Mr. Everitt, was rejected; though whether it was from want of judgment is a subject fully open to discussion.

The advantages which Mr. Everitt represents as to be derived from his own process show, to use his own expression, with what facility an operation may be "bungled." Thus, he says, that if liquor ammoniæ of sp. gr. 0.960 = 10 per cent. ammonia, be wanted, it may be obtained by passing the ammonia liberated from 10 parts of hydrochlorate into 30 parts of water, until they have increased to 35 parts; and then he adds, "that nearly all the ammonia will have passed over." Now, if 35 parts of solution of ammonia, containing 10 per cent. of the alkali, can be obtained from 10 parts of the hydroclorate, not only has the whole of the ammonia passed over and been condensed, but the salt must have yielded more than it contained: for 35 parts of solution must contain 3.5 parts of ammonia, requiring 11.1 parts of hydrochlorate for their production, instead of only 10. I am, Sir, your obedient servant,

R. PHILLIPS.

St. Thomas's Hospital, 11th Jan., 1837.

* We think it right to say that the proof of Mr. Everitt's communication was examined for correction by Mr. Everitt himself. His MS. was correct.

CLASSES IN DUBLIN SCHOOLS.

To the Editor of THE LANCET.

SIR:—In the fifteenth Number of THE LANCET, in an article "from a Dublin Correspondent," the following note is appended to that communication:—"It is notorious that in a school lately opened, the tickets are to be had for any thing, or even nothing, in order to have the *éclat* of a large class at the first start. This is sad work."

As the original Peter-street School is no doubt the Institution alluded to, I feel it incumbent upon me to give the most direct contradiction to this slanderous statement, the only pretext that I can assign, being the fact that the son of the founder, Mr. Kirby, and the son of my respected master, Dr. Duggan, have done us the honour of accepting free tickets to the school. When this compliment was paid to another of Mr. Kirby's sons, by the London Schools and Hospitals, your Correspondent must admit that there was some ground for a free admission in this case.

Two gentlemen besides obtained free admission, under peculiar circumstances, which motives of delicacy prevent me from mentioning; but I assure you that any school in Dublin would have received them under similar circumstances. As regards the charge of underselling, I defy your Correspondent to state the name of a single pupil who obtained a ticket for less than the usual terms.

We have been exceedingly successful in obtaining full classes.* *Hinc illæ lachryma* of your Correspondent.

Allow me, for the sake of the moral, to direct your attention to the following anecdote:—

Mr. Hume asked the late surgeon Richards, of this city, how he was progressing, almost at the outset of his professional career: "Very well," was the reply; "I am now making £500 a-year;" "You are jesting," said Mr. Hume, "it cannot be possible." Some time elapsed, when Mr. Richards called upon his friend Hume, and stated that surgeon ——, the man whom he had esteemed as his well-wisher, had spoken ill of him. The sagacious listener replied, "Go home, Richards, rejoicing; yon are a lucky fellow. I believe *now* that you are making the £500 a-year."

* The names of the pupils of the several classes I shall transmit, in a few days, as an advertisement for the cover of THE LANCET.

As regards another subject—although I have been *ten* years teaching my profession, I am the youngest Professor in the school, save Dr. Bellingham, to whose merits (as the unsuccessful candidate for the chair of Materia Medica at the College of Surgeons) your Correspondent pays a well-merited compliment.

I am quite certain that you have no wish to lend your columns as the vehicle for the "dirty work" of an anonymous correspondent, whose abortive attempts to injure a school, depending on its own resources only, —upheld by no party or corporate authority, are, while reform is in his mouth, but a sorry specimen of *his* liberality. I have the honour to be, &c., &c.

G. T. HAYDEN,
Proprietor of the original Peter-street School.
Dublin, Jan. 9, 1837.

PROFESSIONAL QUALIFICATIONS OF NAVAL SURGEONS.

To the Editor of THE LANCET.

SIR:—While in this country I have deemed it indispensable to be a subscriber to your excellent periodical, to enable me to keep pace with the vast improvements which are yearly being made in medicine; and I have always observed that you, to the best of your ability, have maintained the interests and respectability of every department of the medical profession.

Two letters have lately appeared in the *Times* newspaper, the first of which questions the celebrity of the men appointed to preside over the medical department of the new London University, and in its mention of Sir Stephen Hammick, the writer speaks of him rather contemptuously, as being a *naval surgeon*.

The author of the next letter is anxious to uphold the great talent of Sir S. Hammick, and states, amongst his other claims, that he was never a naval surgeon, but surgeon to a naval hospital.

Interested individuals have been too much in the habit of decrying naval surgeons. Had Sir S. Hammick been a naval surgeon, with his appointment to Plymouth Hospital, (as all naval hospital surgeons now are), I maintain that he would not be the less fitted for the highest appointment.

If we look over the list of those who have been zealous workmen in the science of medicine and surgery, we shall find, both now and in times gone by, naval surgeons amongst them. Sir Everard Home and Dr. Denman added in their various walks to the science. They had been both in the navy. Mr. Lizars, Mr. A. C. Hutchison, and Dr. James Johnson, belong to the navy. I could mention many other names eminent in the profession, and we have good reason to look forward to many more. The system of education now requisite to enter the navy is more comprehensive than that required from candidates at any college of surgeons in the kingdom. I hope Sir William Burnet will live to witness the effects of the system of education which he has introduced.

I am confident that naval surgeons, as such, acquire great decision of practice, whereby a life, which would be otherwise lost, is often saved. A case lately occurred to me, in which I feel that such has been the result.

Six weeks since I was called, eight miles from my residence, to see a man, who, three months previously, had met with an accident, producing simple fracture of the ulna. He had been successively attended, before I saw him, by four medical men. The fracture had, by ulceration at an early stage, become compound, and the man had continually gone on from bad to worse. When I saw him the muscles of the forearm and arm, to the insertion of the deltoid, were all laid bare. The bones, in many parts, were carious, a fistulous sore extended up to the shoulder,—the state of health, from long-suffering, was extremely low. He had severe cough, continual diarrhœa, and profuse perspirations, with all the symptoms of advanced hectic. On the back there was a large sphacelating sore, from lying on it. One doctor ordered one thing, another ordered another. One ordered, in four visits, thirty shillings worth of medicine. "Alas! for what purpose?" you will say, "unless to enrich the apothecary," and, if report be true, thereby himself. When I saw him I only regretted that I had not my instruments, as I was sure that he had but one chance (and that a slender one) of his life. I came next day, and the change was great. He was sinking fast. I took the arm off close to the shoulder-joint. The vascularity, from the previous disease, required nine ligatures. I gave *Tinct. opii* gtts. 50. The man never had a bad symptom, required no more medicine, was up in ten days, and well in a fortnight. Had I not been several years in the navy, but a private practitioner here, I should probably have been influenced by the same fears as those which had affected the surgeons who had seen him before me, and left him to die, for I am certain that death would have been the result in three days. You will do justice, Sir, to an educated body of men in giving this insertion. I am, your's faithfully,

A NAVAL SURGEON.
Youghall, Ireland, Dec. 31, 1836.

MEDICAL WITNESSES ACT.

To the Editor.—SIR:—I feel much pleasure in bearing testimony to the successful operation of the Medical Witness Act. I

have, myself, in only one instance, been summoned; but on the order granted by the coroner for my remuneration being presented to the overseers of Great Marlow parish, the parochial officers did not for a moment hesitate in paying me the fee to which, by law, I am now entitled.

J. E. BRINSDEN, Surgeon.

Great Marlow, Jan. 6, 1837.

NORTH LONDON HOSPITAL.

To the Editor.—SIR:—Will you permit me to state through the medium of your journal, that if "*Philalethes*" be correct in the assertion he makes in your last number, respecting the exclusion of strangers from this hospital, his case forms an exception to the general rule: and that by pointing out his informant on the occasion he alludes to, he will confer an obligation on, Sir, your obedient servant,

WM. LINTOTT.

North London Hospital, Dec. 29th, 1836.

MEETING

OF THE PROVISIONAL COUNCIL AND MEMBERS OF THE

BRITISH MEDICAL ASSOCIATION.

THE first Public Meeting of the Members of this Association will be held at EXETER HALL, in the Strand, at six o'clock for seven, precisely, on Thursday evening next, the 19th inst. We postpone our remarks on the laws which have been framed by the Provisional Council, as we understand that some amendments have been introduced into them, which have not been yet finally arranged.

This is, essentially, a NATIONAL association; it is open to the reception of *all classes* of medical practitioners; but it has been most prudently determined that the President shall always be a surgeon who is engaged in *general practice*, and that all the Councillors shall have been, or shall be, general practitioners.

The thousands of our professional brethren have now only to unite in a spirit of cordial co-operation, under a governing body, thus qualified, constituted, and interested in upholding their rights, and within a very brief period the immense majority of surgeons in this country will find, that they are enabled to take their proper station in society, and obtain from the public the respect and consideration which are due as well to their attainments as to their utility.

Mr. WEBSTER, surgeon, of Dulwich, is the Chairman of the *Provisional* Council, and we doubt not that he will be chairman of the first *elected* Council of the Association. He is a gentleman of great respectability, and of undoubted talent in his profession. We believe that the meeting on Thursday will be attended by an immense number of practitioners, and we may take this opportunity of reminding the young gentlemen who are now pursuing their studies in the Medical Schools, that as *they* are destined to become practitioners, the labours of the Association cannot be devoid of interest to them. It is not by the aged,—it is not by those whose energies have been broken down by time, anxiety, and toil, that great reforms in our institutions can be achieved. We look, therefore, to the junior members of the profession, to the young men whose judgment and spirit of independence ought to admonish them that they ought not to commence or pursue their professional labours, without making the most vigorous exertions to place all the laws relating to medicine upon a *secure* and *perfectly just* basis.

It is intended, we believe, to hold weekly meetings both of the Council and all of the members of the Association, during the approaching session of Parliament, and we have every reason to believe that the business of medical reform will now be undertaken and followed up in right earnest in the metropolis of the British Empire.

HUNTERIAN SCHOOL OF MEDICINE.—Mr. Wardrop's Lectures on Surgery were recommenced on Wednesday evening last, and will be continued on every Monday, Wednesday, and Friday, at seven o'clock, during the remainder of the session.

CORRESPONDENTS.

We received no communication signed *Demonstrator*.

Mr. E. NAIRN has been misinformed. Personally we know nothing of the parties, but we think it may be fairly conjectured that they are fools, or they would not, at this inclement period of the year, become candidates for a habitation in Whitecross-street, or for a cell in one of the workhouses of the new Unions.

ERRATA.—In leader, page 531, line 23 from bottom, for we are compelled to implore their want of information, read *we are compelled to deplore their want of information*.

Mr. Baxter's note, page 537, line 9, for 53 read 52; line 29, for didi read *dedi*.